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THE

MEDICAL JOURNAL OF AUSTRALIA

VOL. I.—10TH YEAR. SYDNEY: SATURDAY, FEBRUARY 17, 1923. No. 7.

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PUERPERAL SEPSIS.[1]

BY T. W. LIPSCOMB, M.B., CH.M. (SYDNEY),

Honorary Surgeon, Lewisham Hospital, Sydney.

Introductory.

ON behalf of this Branch I have to offer a welcome to our fellow-practitioners who are attending the post-graduate course now being held in Sydney. The holding of such courses is very largely the result of the work of the Branch.

I need offer no apology for the subject of this evening's discussion, as it is one in which we are all deeply interested—from the point of view of the patient, the practitioner and the State.

Puerperal sepsis, or, as it should be more correctly termed, puerperal infection, has been recognized under various names from time immemorial. But without going into historical details, the two names that stand out in all that leads up to our present knowledge are Semmelweiss, the Austrian, and our own Lord Lister.

The former was the first observer who, by consistent study and prolonged observation, enunciated the cause of puerperal infection; the latter we honour as being the author of the underlying principles on which the present-day teaching of prevention of infection of wounds has been evolved.

The interest in the subject has been revived by that wonderful contribution of Victor Bonney in opening the discussion on puerperal sepsis in the Section of Obstetrics and Gynæcology at the Annual Meeting of the British Medical Association at Cambridge in 1920. That introductory paper and the subsequent discussion stand as a monument of all that is good in British medicine—the intense desire to tackle problems in a calm and judicial manner and the constant endeavour to correlate the theoretical, statistical and laboratory findings with the clinical signs and symptoms.

That is the spirit in which all true and lasting investigation into disease must be pursued. That is the spirit in which, I think, we meet here to-night, each member adding his grain of sand to the common knowledge, ultimately for the public benefit, even though a certain section of modern lay journalism accuses the medical profession of much that is mean and sordid.

As a starting basis we are all agreed, I take it, that puerperal sepsis, so-called, is a microbic invasion. From the clinical aspect the mains points for discussion are (*i.*) the original source of the causal organism or organisms, (*ii.*) the previous conditions in the mother that make her more liable to attack, (*iii.*) the causes operating at the time of

[1] Read at a meeting of the New South Wales Branch of the British Medical Association on January 12, 1923.

labour to permit such organisms to obtain a foothold, (*iv.*) the various measures of prophylaxis, (*v.*) the clinical courses that may ensue when such an infection has occurred and the appropriate treatment at various stages of the infection and of the different types of infection.

Each and every practitioner who has been in practice for any length of time, must have had opportunity for observing examples of puerperal morbidity. Frankly, I do not believe the man who states that he has never had a maternity patient "go wrong." When morbidity does occur, then it is the attendant's duty to go over all the details of the labour and try to ascertain at what stage his precautions might have failed.

Source of Causal Organism.

The organisms most commonly associated with the more virulent type of infections are streptococci and coliform bacilli or a combination of both; the staphylococcus and gonococcus, on the other hand, are more usually associated with less severe types of disease. Many other organisms have in a lesser degree been found guilty.

The source must be from the surface of the patient herself or introduced from without by contact, if exception be made of the small proportion of cases due to a pre-existing blood infection or to dormant, closed-off sepsis in thrombosed, broad ligament veins, the result of a previous puerperal infection.

I think that the risk of puerperal infection by air-borne sources is negligible. My own view is that the introduction of organisms from without, *id est*, from the attendant's hands, instruments, swabs *et cetera*, is responsible for only a very small proportion of the cases that occur.

The main source is from the patient herself, the organisms from the peri-anal and vulval regions gaining entrance to breaches of surface of the maternal passages or to the raw surface of the placental site.

No matter how scrupulously careful a man may be, every vaginal examination during or immediately after labour must be a potential cause of infection by carrying organisms from the surface up into the parturient canal and nearer to a soil suitable for their growth.

For this reason I would welcome an extended trial of the Bonney method of painting the peri-anal and vulval regions with a 1% solution of crystal violet and brilliant green in equal parts of alcohol and water.

Maternal Predisposing Causes.

What previous conditions are there in the mother that make her more liable to attack by roganisms?

I feel sure that pregnancy, *per se*, by its metabolic changes in the liver, kidneys and other organs, makes a woman more susceptible to bacterial invasion. Any intercurrent disease, such as one of the ordinary, every-day infections, hæmorrhage from piles, albuminuria, alcoholism, overwork beyond her capacity, insomnia, malnutrition from insufficient food, domestic worry, must be borne in mind. Surely any cause such as these, that lower the vital resistance of the patient, must make her more susceptible to attack by virulent organisms?

Recent investigations of ductless glands also make me think that disturbances of the equilibrium of internal secretions might have some share in making a woman more prone to attack.

Actual Factors.

What are the actual factors at the time of labour which permit such organisms to obtain a foothold?

Firstly, the shock of labour itself, combined with certainly some bruising, if not open wounds, of the passages, must to some extent lower that patient's resistance.

Secondly, the exhaustion due to a prolonged labour, especially after an early rupture of the bag of waters, with possibly more frequent vaginal examinations, is a very big factor.

Thirdly, prolonged labour is the more likely to need operative or manual termination and is the more likely to be followed by trauma of the soft parts, leaving an additional surface on which organisms may flourish.

In the fourth place, needless vaginal examination may be the actual factor.

And, lastly, retention of pieces of placenta, of membranes and of blood clot within the uterus favours infection, more particularly placental remains, though this is more common after abortion or premature labour than after confinement at full term.

Prophylaxis.

Prophylaxis must be ever the goal aimed at and be continually in our minds.

In this connexion a most essential factor is an efficient pre-maternity supervision. No point in the mother's condition or surroundings is so small that it can be ignored. I do not wish to dwell too long on this important phase; it has been done so often by much abler men than myself. The advice to the mother about the selection of a suitable nurse or nursing home, about her diet, mode of living, her organic functions, teeth, digestion, the regular examination of the urine and attention to a host of other details, all tend to benefit the patient and to give her that trust in her attendant which is such a big personal factor at the time of accouchement.

I cannot lay too much stress on the necessity for the complete physical examination of the patient at or about the end of the seventh month.

This is the golden opportunity for discovering any abnormality of presentation or disproportion in the size of the fœtus or undue narrowing of the bony canal. Any departure from the normal can then be corrected or, if necessary, induction of labour be advised before term.

The influence that all this has on the subject for discussion to-night is that in most cases labour can be conducted without any pelvic examination or at most only one, thus reducing to a minimum one of the most fertile sources of infection. When operative or manual intervention is necessary, then the attendant must be in a position to carry out these manipulations with as nearly as possible all the aseptic ritual and precautions that he adopts when about to perform an operation.

The preparation of the surrounding surfaces of the patient and the sterilizing of towels, dressings, gloves, instrument *et cetera* must be attended to.

Sufficiently deep anæsthesia by a second medical man, if available, is of importance in lessening shock.

One of the most essential points is that there must be no undue hurrying over the third stage; sufficient time must be given for the placenta to become separated. In this way only can the attendant be sure that no portion is broken off and left *in utero*. If any portion is left, then not only is there soil for bacterial growth, but delay in involution occurs and a cavity is allowed to form in which blood clot may collect, this clot being a further soil in which organisms may grow.

Manual removal of the placenta is not called for unless there is undue hæmorrhage or a persistent rise in pulse rate. If manual removal of the placenta be necessary, an endeavour should be made to remove it while wearing gloves and a sterilized sleeve over the arm. Not infrequently, however, the placenta is veritably "grown to the womb" and the bare hand must be used. Every endeavour should be made to make sure none is left behind to offer a focus for bacterial growth and by retarding involution to favour the retention of clot.

Personally I am not so terrified of manual removal as I used to be. I am of opinion that the non-separability of the placenta is very often the result of some pathological condition of the endometrium prior to conception.

It is essential that wounds of the vagina or the outlet should be cleansed carefully of blood clot and accurately sutured before the patient is left.

Types of Disease and Treatment.

The clinical types of disease which may ensue as the result of infection and the appropriate treatment of each type call for an immense amount of worry and thought. Each individual patient must be studied and dealt with as the individual circumstances demand. But there are various general lines of procedure which I usually adopt.

On the first rise of temperature or of pulse rate it has been my own custom to make a careful examination of the patient to ascertain if there be a cause in any other part than the pelvis. A thorough emptying of the bowel is ordered and a full dose of quinine or some other febrifuge.

At the end of twenty-four hours, if the clinical signs persist, I invariably explore the pelvis under an anæsthetic and make a thorough examination of the passages for unhealthy looking wounds. Any unhealthy wounds of the perineum or vagina are freely exposed, stitches removed and the septic surfaces swabbed with pure carbolic acid. I then examine the cervix and the uterus.

The uterus should be explored with the finger and curetted properly, an endeavour being made to make sure that it is clean. It should be dried out with a swab and then packed with iodoform gauze freely soaked with iodine. I use gauze 7.5 centimetres (three inches) wide in lengths of six metres. The iodine used is one part of *linimentum iodi* to two parts of the tincture. I carry the iodine in a wide-necked, stoppered bottle; the gauze is dipped into that and, without being wrung out, is packed into the uterus.

It is at this early stage that a thorough exploration and cleaning of the uterus in the above manner is so valuable. No more curetting is necessary, nor is it advisable. At this stage it is a most valuable procedure and I am sure frequently aborts the disease.

If, in spite of this, the symptoms and signs increase, then it is certain that the infection has passed through into the deeper tissues of the uterine wall, into the lymphatics or into the veins beyond.

It is at this later stage that curetting is considerably more dangerous, owing to the softening of the uterine wall, on account of the disturbance to Nature's barrier of leucocytes and the possibility of opening up fresh areas on which the infection may seize.

When the course of the disease is characterized by a parametritis with exudation and thickening of the connective tissue of the broad ligament, causing a big, hard mass and fixing the uterus as in a splint, then active local treatment must remain in abeyance and treatment maintained on general lines by rest, warmth and so forth. It may necessitate treatment for from four weeks to many months and tax the patience of the attendant and nurse to the uttermost. If the peri-uterine effusion goes on to suppuration, either tubo-ovarian or in the broad ligament, then it must be treated on general surgical principles and the necessary procedures carried out.

The desperate cases are those which are purely streptococcal in origin. In these patients the lochia is inoffensive and rapidly diminishes in amount. The temperature steadily increases and, although the patient does not feel ill, she is obviously getting worse. In this class of infection, too virulent for any formation of leucocyte barrier, an opportunity occurs for the use of a specific remedy in addition to the usual general treatment.

In my hands vaccine treatment has been disappointing, but I have real faith in polyvalent anti-streptococcal serum, starting with thirty cubic centimetres and giving a lessened dose each day afterwards as the occasion warrants.

Conclusion.

In conclusion, if there is one point that needs emphasis, it is prophylaxis and, in particular, pre-maternity supervision.

There is no royal road to treatment. It is a disease full of vagaries and with many and various phases. There is still room for much investigation and collection of results. Let us always be on our guard and remember that, because recovery in one case follows a certain course of treatment, it is *post* and not *propter*.

If it be permissible to bring up politics at such a meeting as this, we must all deplore the fact that, in spite of the efforts made by this Branch, there is still no statute for the registration and supervision of midwives. It is not too much to expect that the lessening of maternal morbidity and mortality that has followed such legislation in other parts of the world, will also occur here.

ON SOME RELATIONS OF CLIMATE, WEATHER AND FAT COVERING TO METABOLISM.

By C. E. CORLETTE, M.D., CH.M., D.P.H.,
Surgeon to the Sydney Hospital.

CARL VON VOIT, of Munich, and afterwards his pupils, M. Rubner in Germany and W. O. Atwater in the United States of America, were the chief leaders and inspirers of a school of physiologists who have devoted their attention to the study of metabolism on its dynamical side and who have by degrees been bringing its laws into intelligible order and shape. Though the discoveries made have been not only of great intrinsic interest, but also of enormous importance in other ways, the great body of medical men, all ranks alike, for a long time took little interest in them; it is hard to say why. But the great war compelled everyone to take notice and now there are many who are keenly interested and who have an appreciation both of the medical and of the sociological importance of the study. There will, therefore, be readers interested in some of the matter here presented.

Temperature Regulation.

Let us begin by recalling to mind some of the physiological mechanism of temperature regulation. Heat leaves the body chiefly, almost wholly, by passing into the surrounding atmosphere. There are two modes by which this may occur: the first is by radiation and conduction into the air; the second is by vaporization of liquid moisture, the loss of heat being equal to the heat of vaporization of the weight of water whose physical state is changed. On the body surface the water comes from the skin perspiration, which, in the main, means sweat secretion, but in the respiratory passages the moisture is ordinarily derived from the mucous membrane. Under cool atmospheric conditions there is a tendency for heat loss to increase and in that case the organism makes up for the loss by burning more fuel, that is, its metabolism increases so as to keep up the body temperature to the required level. This is called the chemical regulation of temperature. Practically speaking, this is the method of temperature regulation that is proceeding when a man experiences an inconvenient sensation of cold.

But in hot weather the air is so warm already that it cannot conduct away enough heat from the body merely by getting warmed and simple radiation is ineffective. When this is the case, the skin not only becomes flushed to aid more effectually in the dispersal of heat by radiation and conduction, but, in addition, there occurs an active secretion of sweat to provide greater cooling by abstraction of heat of vaporization, the secretion being proportionate to the need. This is called the physical regulation of temperature. Physical regulation would be blocked if the immediate atmospheric environment were so saturated that evaporation could not occur; therefore, some movement in the atmosphere is required so as to carry away moisture-laden air from the body and to bring up a fresh supply of drier air. The effect of wind,[1] even when the current is too slight to be perceptible, is very great, increasing the output of heat per unit of time in a most remarkable way. This effect is diminished by clothing, which slows the air current in proportion to the frictional resistance which it offers. This is the means by which clothing warms the body, whatever be the fabric of which it is composed; by the relative stagnation of the air in immediate contact with the skin this layer becomes warmed up and saturated with moisture, so as to approximate to the condition of a tropical atmosphere, practically at a temperature of 33° C. (91° F.), which is the "critical point" where neither chemical nor physical regulation is called upon. This is the condition which every individual instinctively tries to reach in the effort to attain comfort and conserve heat loss. When the weather itself is of a tropical character, more movement is required in the environing layer of air in order to carry away heat and therefore lighter and more open clothing is worn. Except by variation of clothing, which is not a natural physiological mechanism, no regulation of the wind effect is possible for the skin surface. The regulation of heat loss from the skin by the body's own intrinsic physiological mechanism is carried out, as we have seen, by modifications of blood supply and of sweat secretion. Respiration, however, is subject to other conditions; here it is not the heat and moisture supplied to the parts, but the respiratory ventilation current—corresponding to the wind—that is subject to physiological variation.

Respiratory Heat Loss.

During the performance of external muscular work metabolism is increased and heat production rises. There is usually a surplus beyond what is required to maintain the normal temperature and this surplus heat has to be got rid of. The heat-losing mechanism of the skin is put into action. At the same time the respiratory movements become more active, increasing the physiological wind current over the surface of the respiratory tract, thus increasing also the respiratory heat loss.

The effect of the air on heat loss by the skin depends on its temperature, its vapour content, its content of fog or mist (liquid water in suspension) and the wind velocity. The effect on heat loss by respiration depends on its temperature and vapour content and it is not influenced by water in suspension (except in very small degree) nor by wind. It is usually stated that a moist, cold air is a much severer environment than a dry, cold air, but this is a statement that requires some qualification, since it is either true or not true according to the meaning attached to the term "moist." The lower the temperature of the atmosphere, the less water vapour it can hold and a few degrees below freezing point it is nearly dry. But, provided its temperature is above that point, the drier the air, the more water vapour it can absorb. There may, indeed, be but little skin perspiration at low temperatures, but what little there is is more easily evaporated in contact with dry air than with air already contain-

ing some water vapour. Therefore, cooling by evaporation will be enhanced by dryness and evaporation is a much more potent mode of cooling than that effected by warming the air, other things being equal. In the same way, heat loss by respiration must also be much increased by dryness and decreased by presence of water vapour in the air inspired. Provided, therefore, that the moisture is present in the form of vapour, the conclusion is certain that cold, moist air is a milder environment than cold, dry air.

But if, on the other hand, the moisture is in the liquid state, present in suspension, as in fog or mist, the conditions are altered. Then the suspended liquid is brought into contact with the clothing or with the skin and the heat of the body is drawn upon, first to warm and then to vaporize it. For every gramme of liquid water converted into vapour, 0.58 of a calorie is abstracted from the body heat, or sixteen and a half calories for every fluid ounce. It will be seen at once that a strongly refrigerant process is brought into action, causing a loss of heat in proportion to the weight of water vaporized, just as it is in the presence of sweat evaporation. If the liquid moisture is so great as to be equivalent to a "wetting," loss by conduction also is promoted and if in addition there is a wind blowing, the loss of heat by these two modes may be enough to put a harmful strain on the mechanism of temperature adjustment, perhaps temporarily lowering vital resistance and exposing the subject to infective influences that he might in more favourable circumstances have been able to resist. This is a "chill" and the subject "catches cold." That evil consequences of this sort do occur is a matter of common experience.

I have said that heat loss in respiration is not influenced by water in suspension, except to a very slight degree. When foggy air is inspired, it is warmed and then expired in a condition of vapour saturation. The only extra heat loss is that involved by the warming up of the liquid, not by its vaporization. It makes no difference to the total heat loss whether the water vaporized to produce saturation is drawn from the moist mucous membrane or from the water in suspension. The only difference to the body is a diminution in water loss.

Rubner[2] does not seem to have appreciated the fact that moisture in suspension tends to cause extra heat loss *viâ* the skin surface through vaporization. In explaining why in a dog a "cold, damp environment of 0° C. to 5° C." (which must have meant a foggy one) favoured increased metabolism, he attributed this action of humidity to the increased conductivity of a hair covering containing moisture and he remarked that this loss might have been partially balanced by a decreased evaporation of water from the lungs.

I now proceed to consider a side of climatic action that heretofore does not seem to have attracted much attention and to set forth what seems to me a very interesting method of investigation by which certain factors in the action of climate or weather can be measured and expressed numerically.

Some time ago I was involved in the study, experimental and otherwise, of the heat losses connected with ether anæsthesia. As part of my study I devoted myself to the question of measuring the heat loss that might occur through the respiratory system. I found that it was possible to work this out by calculation when the temperature and hygrometric condition of the inspired air were known and the volume of the respiratory ventilation current measured. The calculation assumes that the measurement of the air respired corresponds to a temperature of 33° C. and that it is fully saturated with moisture. I will not take up space by discussing the reasons for these assumptions as to temperature and moisture content, though I may say that it had been supposed up to 1912 that the temperature of respired air was 37° C..

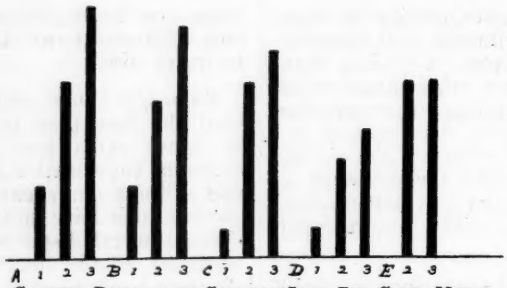
Calculating to one cubic metre (one thousand litres), I show in Table I. the actual figures in large calories for dry and saturated air respectively at 0° C. (32° F.), 20° C. (68° F.) and 33° C. (91.4° F.) and the same results are next shown graphically in a diagram which exhibits the facts in a way more striking than numerals.

TABLE I.—HEAT LOSSES IN RESPIRATION PER CUBIC METRE MEASURED AS EXPIRED.

Weights in kilograms, heat in large calories. Values for water-vapour calculated from weights as given in Smithsonian Meteorological Tables, fourth revised edition, 1918, Table 74.

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hygrometric Condition When Inspired.	Temperature.	Weight of Air Inspired, Exclusive of Vapour.	Heat Required to Warm No. 3 to 33°.	Weight of Water Present.	Heat Required to Warm No. 5 to 33°.	Total Heat Required to to Warm to 33°.	Water Required to Saturate at 33°.	Heat Required to Saturate at 33°.	Total Heat Required to Warm and Saturate at 33°.
Dry ..	0° C.	1.0967	8.60	0.0000	0.00	8.60	0.0356	20.65	29.25
Saturated	0° C.	1.0967	8.60	0.0041	0.07	8.67	0.3515	18.25	26.94
Dry ..	20° C.	1.0967	3.39	0.0000	0.00	3.39	0.0356	20.65	24.04
Saturated	20° C.	1.0967	3.39	0.0161	0.10	3.49	0.0195	11.31	14.80
Dry ..	33° C.	1.0967	0.00	0.0000	0.00	0.00	0.0356	20.65	20.65
Saturated	33° C.	1.0967	0.00	0.0356	0.00	0.00	0.0000	0.00	0.00

Table I. and the graphs show at once that in respiration by far the greater part of the heat loss is caused by the vaporization of water and that the mere raising of the temperature is comparatively unimportant. Table I. also shows that the heat loss involved in warming up the water vapour already present in the atmosphere is so small as to be entirely negligible in all ordinary calculations. From the fact that air is comparatively dry at 0° C., it follows that the greater heat loss in respiration belongs to the interval between 33° C. and 0° C.. The added heat loss through breathing air as much as 33° below zero, that is, at polar temperatures, is only that required to warm up to 0° and to add the comparatively small amount of water vapour which air can hold at that temperature. In point of fact, while it takes only eleven calories to raise one cubic metre of air from — 33° to 0° and to saturate it with water vapour, it takes twenty-seven calories to raise it further to 33° and to saturate it correspondingly for that temperature. This is very interesting from a climatological point of view; its effect is to save quite a considerable amount of heat loss, say, some three hundred calories or more per day.



GRAPHIC DIAGRAM OF CALORIES LOST PER CUBIC METRE OF AIR RESPIRED.

1, by warming; 2, by moistening; 3, total. A, dry at 0°; B, saturated at 0°; C, dry at 20°; D, saturated at 20°; E, dry at 33°.

The Volume of Air Respired.

We may now inquire what is the volume of air normally respired by a man in a given time under various conditions. There have been a considerable number of experiments carried out by various investigators in reference to this point, but I have selected for my present purpose a table by Douglas and Haldane,[3] being measurements of Douglas's own respiratory ventilation current. This table is convenient and seems near enough to other experimental results to afford reasonable approximations for practical use. The table in the original gives the volume per minute, but I have inserted a column, calculated by myself, showing the equivalent per hour. The figures of the table were calculated to a temperature of 37° C.. As the error thus involved is only small, I have not troubled to recalculate for 33° C..

Of course, with a table like the one given (Table II.), we can only expect to get a very rough idea of the facts, but for all that it is a very valuable collection of information. By breaking up the twenty-four hours into periods of varying respiratory activity and summing up, one can arrive at some sufficiently reasonable approximations for various type-individuals. I have in this way estimated that a man engaged in a sedentary occupation would have a total respiratory ventilation current of something like sixteen cubic metres per day of twenty-four hours and a man following an occupation involving active muscular work would reach a figures of from twenty to twenty-five cubic metres per day. A man lying in bed awake under conditions similar to those obtaining when minimum metabolism is measured, would have a current under, but not unreasonably far from, about ten cubic metres per day, which happens to be a very convenient figure for calculations. I propose to return to this subject later.

Heat Loss and Climate.

It occurred to me that the facts displayed in Table I., since they exhibited a general truth, must have a far wider application than the mere measurement of respiratory heat loss during ether anæsthesia, for which the table was originally designed and constructed. It seemed to me to afford a

TABLE II..

Conditions.	Litres of Air Breathed per Minute at 37° Moist and Prevailing Barometric Pressure.	Respiratory Ventilation Current per Hour in Litres.	Number of Breaths per Minute.	Volume of Each Breath in Cubic Centimetres Moist and Prevailing Barometric Pressure.
Rest in Bed	7.67	460	16.8	457
Rest (Standing)	10.4	624	17.1	612
Walking—				
Two Miles an Hour in Laboratory	16.3	978	12.7	1,296
Two Miles an Hour on Grass	18.6	1,116	14.7	1,271
Three Miles an Hour in Laboratory	20.9	1,254	14.9	1,433
Three Miles an Hour on Grass	24.8	1,488	16.2	1,535
Four Miles an Hour in Laboratory	29.0	1,740	14.4	2,010
Four Miles an Hour on Grass	37.3	2,238	18.2	2,064
Four and a Half Miles an Hour in Laboratory	34.2	2,052	17.2	2,055
Four and a Half Miles an Hour on Grass	46.5	2,790	18.5	2,524
Five Miles an Hour in Laboratory	51.3	3,078	18.3	2,810
Five Miles an Hour on Grass	60.9	3,654	19.5	3,145

quantitative method of examining the connexion between seasonal or other climatic conditions and respiratory heat loss at various places and this opened up a new avenue for investigation. The data required for the purpose are the mean temperature and the mean absolute humidity for the place and period. In order to see what sort of results might be obtained by this method, I have collected data and worked out the figures for a number of places shown in the tables fol-

TABLE III.—MEAN HEAT LOSS IN RESPIRATION PER CUBIC METRE RESPIRED AT VARIOUS PLACES.

In progressive order from lowest to highest. The New York and Chicago figures are for the year 1917 only.

Place.	Mean Annual Temperature (Centigrade).	Mean Annual Absolute Humidity, in Grammes Per Cubic Metre.	Mean Heat Loss in Calories Per Cubic Metre Respired.		
			By Warming the Air.	By Moistening the Air.	Total.
Cairns	24.7	16.1	2.2	11.4	13.6
Rio de Janeiro..	23.3	16.3	2.5	11.3	13.8
Townsville ..	24.3	14.9	2.3	12.1	14.4
Rockhampton ..	22.8	13.7	2.7	12.8	15.5
Brisbane	20.5	12.2	3.2	13.7	16.9
Sydney	17.2	10.4	4.1	14.8	18.9
Melbourne	14.7	8.5	4.8	15.9	20.7
London	9.8	7.3	6.0	16.6	22.6
New York.. ..	10.9	9.2	5.7	17.3	23.0
Chicago	5.9	5.4	6.2	17.7	23.9

TABLE IV.—HEAT LOSS PER CUBIC METRE RESPIRED IN WINTER AND SUMMER SEASONS RESPECTIVELY AT VARIOUS PLACES.

In the Northern Hemisphere the mean of December, January and February is taken as winter and that of June, July and August as summer. In the Southern Hemisphere the seasons are reversed. The New York and Chicago figures are for the year 1917 only.

Place.	Season.	Mean Temperature for Season (Centigrade).	Mean Absolute Humidity for Season, in Grammes Per Cubic Metre.	Heat Loss by Respiration Per Cubic Metre in Calories.		
				By Warming the Air.	By Moistening the Air.	Total.
Cairns (Queensland)	Winter	21.4	13.6	3.0	12.9	15.9
	Summer	27.4	19.0	1.4	9.7	11.1
	Difference	6.0	5.4	1.6	3.2	4.8
Rio de Janeiro..	Winter	20.9	14.1	3.2	12.6	15.8
	Summer	25.9	19.2	1.8	9.6	11.4
	Difference	5.0	5.1	1.4	3.0	4.4
Townsville (Queensland)	Winter	20.0	11.2	3.4	14.3	17.7
	Summer	27.6	19.1	1.4	9.6	11.0
	Difference	7.6	7.9	2.0	4.7	6.7
Rockhampton (Queensland)	Winter	17.3	11.2	4.1	14.3	18.4
	Summer	26.9	17.1	1.6	10.8	12.4
	Difference	9.6	5.9	2.5	3.5	6.0
Brisbane	Winter	15.4	7.2	4.6	16.6	21.2
	Summer	24.8	14.7	2.1	12.2	14.3
	Difference	9.4	6.5	2.5	4.4	6.9
Sydney	Winter	12.0	8.0	5.5	16.1	21.6
	Summer	22.0	13.1	2.8	13.1	15.9
	Difference	10.0	5.1	2.7	3.0	5.7
Melbourne.. ..	Winter	10.0	7.5	6.0	16.5	22.5
	Summer	19.2	9.3	3.6	15.4	19.0
	Difference	9.2	1.8	2.4	1.1	3.5
London	Winter	4.1	5.4	7.5	17.7	25.2
	Summer	16.1	10.0	4.4	15.0	19.4
	Difference	12.0	4.6	2.1	2.7	5.8
New York.. ..	Winter	— 0.1	2.6	8.6	19.1	27.7
	Summer	21.9	13.9	2.9	12.7	15.6
	Difference	22.0	11.3	5.7	6.4	12.1
Chicago	Winter	— 3.3	2.3	9.4	19.3	28.7
	Summer	21.4	12.0	3.0	13.8	16.8
	Difference	24.7	9.7	6.4	5.5	11.9

lowing. For the temperature and humidity data I am indebted to the courtesy of Mr. D. J. Mares, of the Sydney Weather Bureau. As regards the absolute humidity, the data for London are calculated from the hourly mean, but for other places the mean is derived from either the 8 a.m. or the 9 a.m. readings. Mr. D. T. Sawkins, Statist to the New South Wales Board of Trade, has very carefully examined the value of the 9 a.m. readings for London and made a large number of calculations which go to show that the 9 a.m. readings give a really close approximation to the twenty-four hours mean and it is assumed that the relation holds for other places. If this were not so, one would be stranded, for full twenty-four readings are only obtainable for such places as London. It is a remarkable fact that however much the relative humidity, that is, the percentage of saturation, may vary in the course of a day, the absolute humidity, that is, the weight of water vapour per cubic metre, varies very little. Another assumption I have made is that the mean of the temperature readings gives an approximation to the real mean temperature. In my calculations I have ignored the specific heat of the contained water vapour, since Table I. shows that it is a negligible factor in work of this kind. The specific heat of dry air is taken as 0.237.

For a man engaged in a sedentary occupation and respiring sixteen cubic metres per day the differences for some of the places come out as follows:

TABLE V.—MEAN DAILY HEAT LOSS BY RESPIRATION IN VARIOUS PLACES FOR A TYPE-INDIVIDUAL RESPIRING SIXTEEN CUBIC METRES PER DAY.

For the year, for winter and for summer. The New York and Chicago figures are for the year 1917 only.

Place.	For the Year.	For Winter.	For Summer.	Winter-Summer Difference.
Rio de Janeiro..	222	253	182	71
Sydney	302	345	254	91
Melbourne	331	360	304	56
London	362	403	310	93
New York	368	443	249	194
Chicago	382	459	262	197

Basal and Minimum Metabolism.

The minimum metabolism is that which proceeds when the subject is economizing his fuel consumption to the utmost. For standard purposes it is presumed to be that found when the measurement is made with the subject at complete rest in bed after waking in the morning, with the surrounding temperature warm enough to make heat loss from the skin negligible so far as physiological regulation is concerned, and after fasting since the previous afternoon. Fasting is necessary, because food, by its "specific dynamic action," causes a considerable increase in metabolism continuing for about twelve hours. The minimum metabolism varies directly with the surface area of the individual and in a very regular manner. Hence, when it is expressed as calories lost per square metre of body surface per hour it gives (subject to certain qualifications) a method of comparing the metabolism of different persons. The special term "basal metabolism" usually applies to this ratio, but sometimes it is stated in terms of calories per kilogram of body weight. The metabolism of subjects measured in the northern United States is usually not far from 39.7 calories per square metre per hour, or from twenty-four to twenty-five calories per kilogram per day, corresponding to from 1,680 to 1,700 calories for a man of seventy kilograms (eleven stone). As I have already said, we can probably estimate the respiratory ventilation current of a man under conditions of the sort at the rate of something, roughly, near ten cubic metres a day. If we look at the heat losses per cubic metre respired as shown in the three last columns of Tables III. and IV., we can arrive at an approximation to the respiratory heat loss per day for such a type-individual by the simple method of moving the decimal point one place to the right. New York (year 1917) would thus give him a respiratory heat loss amounting to two hundred and thirty calories per day, *id est*, at the rate of two hundred and thirty calories per day.

The Effect of Artificial Heating.

Now let us inquire what are the atmospheric conditions that will prevail in an inhabited room heated by a fire or other means. They will be very different according to the system of warming employed and according to the concentration of living inmates. The more people congregated in a room, the more moisture is imparted to its atmosphere from skin and breath. The rapidity of the ventilation current determines the rate at which this moisture-laden atmosphere is removed. If it is slow, there is more of this moisture retained; if it is fast, then there is less.

When the system of heating is by a fire burning in an open grate, the heat is transmitted to the room by the process of radiation and in conformity with well-known laws. Heat rays proceed always in straight lines through space until they strike a non-conducting substance and then they are either reflected or absorbed by it. If the substance absorbs rays, it becomes warmed. The quantity of heat reaching an object varies inversely as the square of the distance from the source of the rays. Air transmits rays easily and therefore does not become warmed. But when the rays strike the bodies of people in the room or articles of furniture or the walls, the parts that receive them, absorb them and become warmed, but only the parts directed towards the source emitting the rays or towards parts that reflect them. There is a certain amount of secondary warming of the air by contact with warmed walls and furniture, but in open grate warming there is always a very strong current in the atmosphere towards the fire, making accumulation of heat by secondary warming difficult and also preventing accumulation of moisture from the bodies of inmates. One may add that this strong air current tends also to abstract heat from the body surface of inmates, just as it does from furniture, and so heat loss from the skin is greater than it would be in a current of lower velocity. Summing up, we we find that people breathing the air of a room warmed by an open grate are breathing practically the same atmosphere as that outside, both as regards temperature and humidity, and whatever the effect of the outside air may be when breathed, it

is not avoided to any material extent by being indoors.

Let us consider next the case of an inhabited room warmed by a stove, hot-water coil or steam pipe. In this form of heating, radiation plays only a minor part. The air itself is warmed by passing over the hot surface and, being warmed, it ascends and a current forms and circulates throughout the room, exactly as currents form in a pot of water placed over a fire, the room corresponding to the pot, the air to the water, the stove to the hot bottom of the pot. Such current are called convection currents and the system is one of heating by convection. The current of air towards a stove is very gentle as compared with that to an open fire and only a very small portion goes to feed the fire and pass up the flue. With convection methods of heating there is probably a little more absolute humidity in the room air—derived from the bodies of inmates—than when radiant heat is employed, though its drying power will be great because, notwithstanding a little more absolute humidity, it is far from being saturated. But the absolute humidity would not be increased in anything like proportion to its increase of temperature. We may say that the person breathing such an atmosphere inspires air that is probably just a little more humid—considered absolutely—than the outside air and a great deal warmer. The influence on respiratory heat loss depends far more on the absolute humidity than on the temperature, as is shown by Table I. and the graphs. Therefore, the climatic effect is only very moderately altered in that respect, even if the air be made quite uncomfortably warm. It would, however, be greatly modified if moisture were copiously added in addition to the mere warming.

The Effect of Wind.

As we have already seen, people instinctively try to procure for themselves an environing layer of air beneath their clothing at about the temperature of 33° C.. It should be added that, by means of the perspiration of moisture through the skin, this environing layer will tend to be kept in a moisture-saturated condition, or as near to it as possible. Thus it is brought about that the air beneath the clothing approximates both in temperature and in hygrometric condition to the condition of expired air. The formation of convection currents and the perflation of wind through the clothing, together with diffusion, provide the substitute for respiratory movements.

The heat loss figures of Table I., Table III. and Table IV. signify the refrigerating or heat-lifting power per cubic metre of the kinds or samples of atmosphere there dealt with within the 33° limit of temperature. While it must be admitted that sometimes wind temperatures may be over 33° C., tables of the sort are capable of employment for rather more than mere respiratory heat loss purposes. We can use them for effects on the body surface. If we wish to consider the heat lifting or refrigerating power of a moving atmosphere, we can leave out of our calculations the respiratory ventilation current and substitute wind velocity in metres per second. This will give us an expression of potentialities for refrigeration, not actual heat loss, but what it could be if the heat-lifting power were fully exerted. In this way we acquire a numerical method of expression by which we can compare two different specimens of wind. If we could multiply the figures in Tables III. and IV. by the mean wind velocity per metre per second for the corresponding periods, we would get a numerical expression of this sort for season or climate which seems to present possibilities of usefulness. However that may be, it gives us at least an anchorage for ideas, allowing an orderly presentation of facts and an opportunity for survey. The customary figures showing relative humidity throughout the day, stated as a percentage of saturation, lose none of their value. But it would seem that a broader view of the whole situation is provided when we have both modes of expression available, since a larger number of factors are included. Of course, from what we have already seen, a numerical statement calculated for air and its content of water in the physical state of vapour alone would not suffice for a fog-laden atmosphere. If we knew the weight of liquid water in suspension in a given volume of foggy atmosphere, it would be possible to add a factor corresponding to the quantity of heat that would be absorbed in vaporizing it. It should be remembered also that wind-velocity effects are effects on the clothed body. But here it may be repeated again that actual heat loss is not supposed to be indicated by the methods suggested; any numerical statement would indicate potentialities only—which is its intention—and that is quite sufficient if we only wish to compare one atmospheric condition with another. Measurements of minimum metabolism have been chiefly made in people accustomed to cool climates, such as that of the northern United States and, when arrayed for comparison by calculating as basal metabolism, the figures obtained have been remarkably uniform. Nevertheless, there have been indications tending to show that there was something more in it than could be explained simply in terms of surface or height and weight correlation. For example, it has been found that the basal metabolism tended to be higher in athletes. This has seemed to support the views of those who regard the rate of metabolism as a function of the mass of active protoplasmic tissue rather than as a function of surface area. But if certain recent experimental work done at Rio de Janeiro be confirmed, the quantity of heat produced by different individuals at rest and fasting, as when minimum or basal metabolism is measured, certainly does not depend on the mass of active protoplasmic tissue, nor does it depend only on their skin area, but also on the habitual intensity of their heat production in the past. I think I can show that one or more additional factors have to be considered, but this will be discussed later.

Acclimatization.

The Rio de Janeiro work that I have referred to is that of Homero Lobato and A. Ozorio de Almeida.[4] The latter is Professor of Physiology at the Faculty of Medicine in that city, the former was

one of his pupils and the original experimenter. Lobato made some studies which brought out some very remarkable results, revealing figures for basal metabolism very much lower than those found prevalent in North America. According to de Almeida, these results are recorded by Lobato in a paper of which he (de Almeida) gives the title, but it does not seem to be available for independent reference. Lobato is said also to have presented a note on the subject to the Brazilian Society of Sciences. Upon this, de Almeida took up the study himself. He carried out twenty-six experiments on ten subjects, all white men born in Brazil and nearly every one of them habitually accustomed to athletic exercises. The mean of his twenty-six experiments gave a basal metabolism of 30.35 calories per square metre per hour, with a range in the means of the different subjects from 25.67 to 37.56. The conclusion drawn is that acclimatized white inhabitants of warm countries have a basal metabolism very much lower than that of inhabitants of the colder countries. If confirmed, it shows that the human species has a power of acclimatization exhibited by lowering heat production. This is a new discovery in physiology and one of profound importance.

But in regard to this discovery the ordinary reader might easily fail to suspect what is really implied by the rather bald-looking statement that the mean result of de Almeida's measurements showed a basal metabolism of 30.35 and so on, so I will assist him by developing it and translating it into other terms. We have seen that the metabolism of a man in a colder climate with a basal figure of about 39.7 would mean for a person weighing seventy kilograms a total heat production amounting to some 1,680 calories per day. Now, for the same man, a basal metabolism of 30.35 would mean that he only produced 1,290 calories per day under otherwise similar conditions, involving a reduction of 390 per day. The lowest metabolism in de Almeida's subjects was no more than 25.67 and this would for the same seventy-kilogram man be equivalent to about 1,090 calories per day, a reduction of 580 on the normal metabolism of the cooler climate. These are relatively enormous differences.

But there is still more in these figures than at first meets the eye. Let us now translate into terms of sweat evaporation. We find that a reduction of 390 calories a day means that the individuals concerned would be spared the evaporation of over 672 cubic centimetres (equal to 23.6 British or 22.7 American fluid ounces) of sweat water per day as compared with the unacclimatized North American placed in a hot climate. The individual with a metabolism of 25.67 would be spared the evaporation of no less than 1,000 cubic centimetres (equal to 35.2 British or 33.8 American fluid ounces) of sweat water per day. This is a form of acclimatization never before suspected. It is not difficult to imagine what an enormous difference in comfort and in physiological effort this represents and also what an advantage is conferred on the individual thus acclimatized, in the lessened liability to heat stroke and in the greater powers of mind and body likely to be derived from such a relatively unhampered condition of existence. It is clear that we have here a new field to explore and one that may yield most valuable results. Perhaps our institutes of tropical medicine could collaborate in a wide research on the subject. It means a great deal to a country such as Australia, which aims at solving the problem of settling a white population in tropical regions.

In Table I., which displays the range of possibilities of heat loss by respiration, we find that between a dry atmosphere inspired at 0° C. and a saturated one at 33° there is a difference of twenty-nine calories per cubic metre in the heat loss. I think I have also shown that the effect may not be greatly changed by being indoors and even when the air inspired has been warmed, the effect on heat loss through respiration will not be altered very profoundly, provided it has not also been moistened. If we can be allowed to estimate the respiratory ventilation current associated with minimum metabolism at somewhere about the rate of ten cubic metres per day, the extreme possible heat loss by the respiratory channel, at temperatures not below 0° C., becomes 290 calories per day and according to the type of atmosphere breathed we could get any heat loss between that figure and zero. In discussing the measurement of minimum metabolism and the meaning of basal metabolism, I said I thought I could show that, not only skin area—an already well-known factor—not only habitual intensity of heat production in the past—de Almeida's factor—but also another factor had to be considered. What I had in mind was this potential 290 calories per day. It will be seen that this factor happens to be very variable, anything between zero and 290, and it is large enough in its higher possibilities to affect results materially unless guarded against in the experiment or allowed for in calculation. It is not unlikely that this new factor is materially involved in the difference between measurements made at Rio and those obtained, say, at New York. That it could only account for a portion of the difference is true, but it must be considered if there at all.

The Effect of Subcutaneous Fat on Metabolism.

With some diffidence I would now like to suggest what seems to me a new point of view in connexion with certain observed variations in the basal metabolism of some classes of individuals. It is put forth merely as a hypothesis for examination and it cannot be any more than that at present. Moreover, it hangs a good deal on the soundness of de Almeida's conclusions and it would lose much of its force if his work or his reasoning cannot be confirmed. The name of Dr. F. G. Benedict has for a long time been prominently associated with the view that the mass of active protoplasmic tissue was responsible for the differences of metabolism observed under circumstances otherwise apparently equal. With Benedict have been associated other eminent co-workers who hold a like opinion. There is no need here to go extensively into particulars and my present purpose will be sufficiently served by using a short passage from a book by Benedict and Carpenter.[6] It is as follows:

Setting aside all differences in physical characteristics and in sex of two people of the same body weight and general conformation, the taller person will have the less subcutaneous fat and women as a class have a larger amount of subcutaneous fat than men. Consequently the active mass of protoplasmic tissue would be somewhat less with women than with men and, if this is the controlling factor of metabolism, we would expect a lower metabolism. We would also expect that the values obtained on women would compare more exactly per kilogram of body weight with those obtained on fat men.

And they go on to quote figures showing that this is the case.

But if the mass of protoplasmic tissue be not the controlling factor of the metabolism, we might adopt another angle of view and see whether the subcutaneous fat itself may not play a part in controlling the intensity of metabolism in such cases as those considered by Benedict and Carpenter. The idea suggested itself to me on noting an observation of Rubner's,[6] quoted by Lusk. Rubner investigated the influence of temperature on the metabolism of a fasting short-haired dog at a time when he was emaciated and compared it with the fasting metabolism after the same dog had been fattened. The figures showed that the metabolism of the dog was the same at a low temperature in both cases, but that the minimum metabolism was almost reached at a temperature of 22° C., when the dog had a protective covering of fat, which was not the case when he was thin. The conclusion arrived at was that the presence of adipose tissue acted in the same way as does a warm fur to extend the range of the physical regulation and to delay the onset of the chemical regulation of body temperature. We have here in fact evidence that a thick layer of subcutaneous fat protects against heat loss.

Now, although it is true that when surface area is measured according to the "linear formula" of D. and E. F. Du Bois,[7] the lowered metabolism of fat people is much less marked than it appears to be when Meeh's formula is employed and still less than when weight alone is used as the standard of comparison, it seems that their metabolism is still a little lower than that of ordinary individuals.[8] If this be so, might it not be possible that the actively functionating tissues beneath the fatty layer "acclimatize" themselves, as it were, to this environment and lessen the intensity of their metabolism in a way comparable to the acclimatization which appears to occur in white people at Rio de Janeiro? It is not an unreasonable suggestion that if such a thing can occur as an adaptation to the atmospheric environment, it should also be possible as an adaptation to the fatty environment. If this be so, it not only may play a part in the explanation of the lower metabolism of women and of fat men, but it may also to some extent complicate the metabolism of individuals over middle age,[9] since, on the whole, such subjects tend to have a greater accumulation of fat and one might then be able to attribute the lessened metabolism which is observed in such cases, not solely to age, but in some degree also to this sort of "acclimatization," if such a word can be used for the process.

Summing up the factors or apparent factors of the basal metabolism, we find that, instead of the skin area alone, there may be found a series somewhat as follows: (*i.*) skin area, (*ii.*) habitual intensity of metabolism in the past, (*iii.*) varying differences of heat content between the inspired and expired air, (*iv.*) age, (*v.*) protective fatty environment of the actively functionating tissues, (*vi.*) sex.

Heat Loss by Respiration and Heat Loss by Skin.

Before going on to the next section, we may inquire what may be the ratio of heat loss by respiration to heat loss from the skin from all causes, radiation, conduction and evaporation. Some three years ago I worked this out by my own methods for the purpose of discovering the facts in connexion with one of the earlier series of experiments on the metabolism of resting men—not, however, complete rest, as there was a fair amount of movement, though it might be described as sedentary occupation—recorded by Atwater and Benedict[10] when working with the original Atwater-Rosa respiration calorimeter. I did this to satisfy my own curiosity, because it appeared to me that the calculation could be done and so far as I then knew the literature, it had never been done before. I worked it out for several possible conditions of relative humidity and respiratory ventilation current, but I considered that the most probable conditions were a respiratory ventilation current of about 600 litres per hour and a relative humidity of 50% at 20° C.. I calculated the heat losses between 7 a.m. and 7 p.m. and show them here as percentages of the total metabolism for that period: Urine and fæces, 1.9; warming inspired air, 1.7 (includes warming inspired vapour, 0.04); vaporization of water in respiration, 9.2; vaporization from skin, 12.3; radiation and conduction from skin, 74.9; total heat loss by respiration, 10.9%; and by skin, 87.2%. I have since discovered that Benedict[11] had anticipated me in the calculation of the details necessary to such an inquiry. His data were from a different group of individuals whose metabolism was measured in an improved respiration calorimeter adapted for measurement of the oxygen consumption. An approximate estimate of the respiratory ventilation current was obtained by multiplying the volume of oxygen consumed by 21. I based my estimate on Douglas and Haldane's figures and it would not have been possible to calculate from oxygen consumed, as it was not measured in the older calorimeter. However, Benedict arrived at the following percentages: Urine and fæces, 1.2; warming inspired air, 2.3; vaporization of water in respiration, 9.8; vaporization from skin, 12.3; radiation and conduction from skin, 74.7.

The setting out of results in two antithetic groups in order to discover separately the percentages of heat loss *viâ* the respiration and *viâ* the skin and to discover the ratio between them, is not in the original work of Benedict, but as his figures were adaptable to my purpose, I have made that use of them and put them together in parallel with my own. They show that the total heat loss by respiration was 12.1% and by skin 87%.

The very close agreement between my figures and those of Benedict is remarkable; they are almost identical. I think we may conclude that for condi-

tions approximately similar to these the respiratory heat loss is from one-eighth to one-seventh of that from the skin. Furthermore, if we could assume such a ratio to hold, a small difference in respiratory heat loss would portend a very large difference in total heat loss. But during moderately active work or exposure to cold, I imagine that this ratio would alter, heat loss from the skin being proportionately increased.

Metabolism in Sydney.

We should, however, remember that during active muscular work hard enough for a man to keep himself warm, there is no calorie loss through direct chemical temperature regulation, since the individual then uses the heat which forms a by-product of his work to keep his temperature up to normal. So far as it goes, this would tend to lessen the difference in metabolism directly due to variation of meteorological conditions. This applies both to respiratory and to surface heat loss. But during non-working hours the meteorological conditions resume their importance.

If we examine Tables III. and IV., it will be seen that the mean climate of Sydney, when expressed numerically, for still atmosphere, is half-way between that of Chicago and that of Rio. This suggests to me a conjecture that, if other things are equal, the level of metabolism may be found to lie somewhere about a corresponding position between that observed in the Northern United States and that observed at Rio. But since we are at present without sufficient measurements of metabolism for inhabitants of Sydney, the facts must, for the present, remain uncertain.

Though it is possible to calculate approximately as in Tables III. and IV. the seasonal or climatic differences in metabolism so far as heat loss through respiration is concerned, the total difference must include heat loss from the body surface also and this we should expect to be much more. The only estimate in reference to this point that I have come across in the literature available to me is to be found in a pamphlet by Dr. C. F. Langworthy,[12] of the United States Department of Agriculture. Writing, apparently, of North American conditions, he states that it has been estimated that the heat loss (total) in winter is 800 calories per day more than it is in summer. Unfortunately, he does not cite his authority, nor does he give the reasoning on which the estimate was based, but he is unlikely to have mentioned the figure without some reliable authority. Not having anything better, I will use Langworthy's 800 calories as a provisional figure for an American winter difference, though the considerations I have set forth in an earlier paragraph suggest very strongly that it is far too small. However, we can let it stand for present purposes. We can be sure that if 800 calories represent the facts for, say, New York, nothing nearly so great would obtain in the very much warmer climate of Sydney, where the winter is far milder and shorter. A comparison of the figures given in the tables show how great a difference there is.

If, then, we make use of the 800 calorie figure provisionally as roughly approximating to the extra winter heat loss of the colder part of the United States, as at New York, it does not seem possible or probable that more than from a quarter to a third of this loss could have left the body by way of the respiration. My calculations for a sedentary individual breathing sixteen cubic metres a day show a winter-summer difference of 194 calories in New York, thus accounting for a quarter of the 800, leaving about 600 to be dissipated through the skin. But sixteen cubic metre type-individuals would lose by respiration sixty-six more calories per day in the New York than in the Sydney mean atmosphere. We may be reasonably sure that a man losing sixty-six calories per day through respiration would at the same time be likely to lose a much greater quantity of heat through his skin, in spite of clothing. I know no way of getting exact knowledge, but on the figures we have obtained, it would seem but a modest conjecture to place the total extra heat loss due to climate at three to four times the figure standing for difference of respiratory loss alone. If the latter be sixty-six, the total difference of heat loss due to difference of climate or season would, therefore, not be excessively estimated at two hundred calories per day for a more or less sedentary individual. This has a bearing on the relative cost of living, for, over and above the extra cost of clothing, it means that a man living in the colder climate has to eat extra food, corresponding on the year's average to not less than two hundred calories per day beyond the requirement of an otherwise similar individual living in the warmer climate. This means that he would have to buy at least 73,000 food calories a year more in the one place than in the other.

We have seen that in certain calorimeter experiments on more or less resting or sedentarily occupied men the heat loss by respiration was to the heat loss through the skin approximately as 1:7 or 1:8. We do not at present know how far beyond the conditions of the actual experiment such a ratio would prevail, but I have taken the trouble to work out the figures that would be shown by individuals breathing the mean atmosphere of New York and of Sydney respectively if the 1:7 ratio were to prevail. I find that in such a case, the climatic difference would cause a difference in total heat loss for each individual per day amounting to over five hundred calories, instead of the amount (over two hundred calories) estimated in the paragraph immediately preceding.

Figures like those just considered compel thought and should stimulate to experimental methods of study, but they should not be permitted to carry one away rashly to unchecked conclusions. The way to sure knowledge is through patient and laborious experimental work and for this there is no substitute.

Recently, however, there has been some important work published in THE MEDICAL JOURNAL OF AUSTRALIA by Dr. W. H. Halcro Wardlaw[13] which is entirely pertinent to the subject we have just been discussing. It consisted of a series of dietary studies on ten men and nine women, all either teachers or students in the University of Sydney.

The average energy content of the food was 2,170 calories per day for the men and 1,925 calories for the women. The average energy consumption per day per square metre of body surface was 1,330 calories for the men and 1,283 calories for the women. There were no measurements of basal metabolism. The subjects were, on reasonable grounds, considered to be classifiable as individuals engaged in "light work." Those familiar with the figures of food consumption for a similar class of subjects in the northern United States will recognize a really enormous difference. Atwater's famous standards were founded on the study of a very large number of dietaries in the United States.[1] It will be seen that, although the Sydney subjects were classified as at light work, the Sydney women consumed 475 calories less than the standard given for American women doing no muscular work at all and as much as 775 calories less than the standard given for American women with light muscular exercise. The Sydney men consumed 230 calories less than the standard set for American women doing no work, 530 less than the standard given for American men doing no work and 830 less than the standard given for American men supposed to be doing an approximately similar amount of muscular work.

The relatively low figures of the Sydney subjects cannot be explained as due to under-nutrition. Some of the difference could be accounted for as due to their lightness of weight, but that will not take us very far. Yet there must be an explanation. What is it? Does my explanation supply it? That will be for the future to decide. But whether my explanation be the right one or not, it is at present the only one offering. I consider it rather remarkable that I should have forecasted Dr. Wardlaw's results.

Summary.

Provided the moisture is present in the form of vapour, a cold, moist air is a milder environment than a cold, dry air. But when the moisture is present in suspension, as fog, the reverse is true. Foggy air only makes a difference in heat loss through the skin, that lost by respiration being practically unaffected.

In respiration by far the greater part of the heat loss is incurred by the vaporization of water, the mere raising of the temperature of the inspired air being much less important. The potentiality of heat loss by respiration is by comparison much greater between 33° C. and 0° C. than between 0° C. and — 33° C.. If the temperature and hygrometric condition of the inspired air and the volume of the respiratory current be known, it is possible to calculate the respiratory heat loss and to state separately the calories lost by raising temperature and by vaporization of water. It follows that if the mean temperature and the mean absolute humidity of any given place be known, a numerical statement can be given to respiratory heat loss for that place. Furthermore, if mean wind velocity be known, a numerical statement can also be given to the potential value for carrying away heat from the body surface. In the case of a fog-laden atmosphere, if the weight of liquid water in suspension be known, the heat required to vaporize it can be calculated.

This points the way to a scientific method of comparing the physiological effect of two winds or two climates or seasons.

So far as respiratory heat loss is concerned, the climatic effect is probably but little modified when a subject is in a room heated by an open fire. It is modified to a greater extent by convection methods of heating, but even then not very seriously.

There is evidence tending to show that the basal metabolism of white men acclimatized to living in the tropics is very much less than in colder climates. This confers on them very marked advantages as regards their facility for regulating body temperature. For example, it seems that acclimatized white inhabitants of Rio de Janeiro are spared the necessity of secreting some twenty-three and a half ounces of sweat per day as compared with unacclimatized people accustomed to living in the New York climate. The application of this to the "White Australia" question is obvious.

Instead of having to regard area of body surface as the fundamental factor in relation to basal metabolism, we have at least three to consider seriously, (*i.*) skin area, (*ii.*) habitual intensity of metabolism, (*iii.*) varying differences of heat content between the inspired and expired air. If the Rio de Janeiro results can be accepted, the mass of active protoplasmic tissue is not the controlling factor of basal metabolism that some have thought. On the other hand, a thick layer of subcutaneous fat may have an effect in lessening metabolism comparable to that of a hot climate. This may complicate the effect of age over forty, because such subjects are liable to possess more fat. It may also have a bearing on the relatively lower metabolism of women as compared with men.

From the data obtained in certain respiration chamber calorimetric observations on individuals at sedentary occupations it has become possible to supplement the calculation of the separate heat losses incurred (*a*) through warming the inspired air and (*b*) through evaporation from the respiratory passages by a calculation of the heat losses from the body surface, (*c*) by radiation and conduction and (*d*) by evaporation of sweat. For a sedentary individual respiring 600 litres per hour and inspiring the air at 20° C. and at 50% relative humidity the heat dissipated by respiration is approximately from one-eighth to one-seventh of that dissipated by the body surface (as 11% is to 87%).

From various data quoted it has been deduced by calculation that, for an individual engaged in a sedentary occupation, the difference between the colder North American climate, as at New York, and the warmer climate of Sydney probably involves a difference of mean heat loss amounting to at least two hundred calories per day; it may not improbably be over five hundred calories per day. These figures—five hundred calories per day or more—

[1] *Bulletin No. 21*, United States Department of Agriculture, Office of Experiment Stations, page 213:

TABLE 39.—STANDARDS FOR DAILY DIETARY (AMERICAN).

Woman without muscular work	2,400 calories
Woman with light muscular exercise	2,700 calories
Man without muscular work	2,700 calories
Man with light muscular work	3,000 calories
Man with moderate muscular work	3,500 calories

arrived at as a forecast by deduction, appear to be confirmed by recently published dietary studies on Sydney University students, carried out by Dr. W. H. H. Wardlaw, but further work of the same kind is desirable.

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Reports of Cases.

VESICO-VAGINAL FISTULA.

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THE following history is thought worth reporting on account of the difficulty experienced owing to the site of the lesion and the methods which were adopted at the final operation.

L.D., married, aged forty-five years, was seen by me on October 31, 1922. The patient had been married for twenty years, had no children and had had no miscarriages. She stated that she had undergone an operation on February 17, 1922, and that her uterus and both ovaries had been removed. She had not been able to control the urine since the operation. A second operation had been performed for this disability, but no improvement had resulted.

On examination the patient's diapers were found to be saturated with urine. A large vesico-vaginal fistula was present and from this urine was constantly escaping. Examination of the abdominal wall showed the presence of a large incisional hernia. The sac of the hernia apparently contained bowel.

On November 22, 1922, an attempt was made to close the fistula from the vagina. The opening was large and extended laterally for a considerable distance. On account of the close proximity of the ureteral orifices it was decided to abandon the vaginal method of approach. On making an abdominal incision, stomach, transverse colon and small bowel were found in the hernial sac. An incision was made into the bladder and a fine probe inserted into each ureteral orifice. Each ureteral opening was quite close to the margin of the fistula. The opening into the vagina was closed by interrupted sutures of chromacized catgut. This line of sutures was supplemented by a layer of plain catgut sutures. The incision in the bladder was closed. No drainage was used. The incisional hernia was repaired and the patient made an uninterrupted recovery.

Examination on January 11, 1923, showed the patient to be in good health. The result of the operation was perfect.

Reviews.

HYGIENE OF THE TROPICS.

ALTHOUGH our continent is not free from some form of tropical diseases, the incidence of these morbid conditions and possibly their dissemination has been extended in the community as a result of our participation in the late world war. It is but natural, therefore, that our attention should be directed more actively to the prevention of such diseases. In "Aids to Tropical Hygiene," by Colonel R. J. Blackham[1] we have concentrated in a small space an immense volume of information covering the essential range of the subject.

The author has succeeded admirably in effecting a condensation of a subject of this character and he has further avoided the tendency to elaboration of one or more topics at the expense of others, so that the book presents a pleasing uniformity of description.

While we may not be in accord with the writer when he states that it is a mistake to take leguminous seeds as a vegetable with meat or that soft rain water should not be stored in zinc-lined receptacles, still there is so much sound information in the book that we can overlook our disagreements on these points.

In some respects the book would possess a higher practical value if directions given were made more precise. For instance, in employing sulphate of alumina for the clarification of water, we are told to use a 2% solution, but no indication is afforded as to the absolute amount to be added.

Mistakes appear to be few, although the statement on page 7 that "saturated air needs only a comparatively high temperature to produce condensation" is obviously a slip.

We feel that the book can be commended for the use of those for whom it was written.

GONORRHŒA AND ITS COMPLICATIONS.

THE third edition of Dr. George Luys's text-book on "Gonorrhœa and its Complications"[2] is in general character similar to the two previous editions. The text has been revised in many places and new figures and coloured plates have been added. The author has wisely emphasized the dangers of gonorrhœa and its complications.

A chapter has been devoted to inflammation of the urethra due to causes other than to the gonococcus. The anatomy of the urethra and the pathology of gonorrhœa are briefly described. The symptomatology of acute gonorrhœa has been fully discussed. A brief space has been given to a description of gonorrhœa in women and children. The treatment of both acute and chronic gonorrhœa have been carefully described. The chapter dealing with urethroscopy and urethroscopic treatment constitute the main feature of the book. The figures and coloured plates are excellent. The book is an excellent text-book for the use of the students and the general practitioner.

[1] "Aids to Tropical Hygiene," by R. J. Blackham, C.B., C.M.G., D.S.O., M.D., F.R.F.P.S., M.R.C.P.E., D.P.H., with a Preface by Sir John Goodwin, K.C.B., C.M.G., D.S.O., K.H.S., F.R.C.S.; Second Edition; 1922. London: Baillière, Tindall & Cox; Foolscap 8vo., pp. 248. Price: 4s. 6d. net.

[2] "A Text-Book on Gonorrhœa and its Complications," by Georges Luys; translated and edited by Arthur Foerster, Captain, R.A.M.C. (T.C.), M.R.C.S., L.R.C.P. (Lond.); Third Revised Edition; 1922. London: Baillière, Tindall & Cox; Royal 8vo., pp. xvi. + 400, with 212 illustrations and five coloured plates. Price: 21s. net.

The Medical Journal of Australia

SATURDAY, FEBRUARY 17, 1923.

The Australasian Medical Publishing Company, Limited.

THE members of the six Branches of the British Medical Association in Australia have grown accustomed in the course of over eight and a half years to the weekly appearance of THE MEDICAL JOURNAL OF AUSTRALIA. They regard it as their journal, but few trouble their heads concerning the actual relationship of the journal to the Branches. The history of the Australasian Medical Publishing Company, Limited, does not appear to interest them. They recognize that at present the publication of the journal is the only activity of the Company, but very little is known of the other objects for which the Company was formed. In 1913 the constitution of the Company was framed to enable it to undertake a large variety of business and other activities. It is true that the publication of THE MEDICAL JOURNAL OF AUSTRALIA was and remains the chief and most important function of the Company. But the organization was the child of the medical profession in the Commonwealth; anything, everything that is calculated to further the interests of the profession, that may safeguard its dignity and that could enhance its utility to the community would of necessity be a function of the Company. It was born within the British Medical Association, the representative organization of the medical profession. Each of the six Branches elected three of its members to be members of the Company, while one of each group of three became a Director. In this way the management of the Company was vested in the representatives of the Branches of the Association. THE MEDICAL JOURNAL OF AUSTRALIA was destined to serve the Branches severally and collectively. It came into existence in 1914 as an instrument of technical education, as a means for moulding professional opinion and for guiding public opinion on matters concerning the health of the people of Australia and as a piece of machinery for the linking up of the medical profession in all parts of this vast island continent. The organization of this new possession of the medical profession needed to be stable, permanent and capable of meeting its ever-changing requirements. Its business foundation had to be sound, for stability and permanence necessarily depended on the exploitation to its full value of the publication as an advertising medium. At the same time it was essential that the journal should be read widely, both at home and abroad. No journal will command popularity unless the quality of its contents is of a high order, but authors of articles of scientific merit will not utilize a given journal unless evidence can be produced that the articles are being read with care. It was in consequence necessary to plan the journal so that the interest of members would be insured and that the best work completed in Australia would find its way into its columns as a matter of course.

The utility of the journal is limited by the income it can earn. The revenue derived from advertisements has increased steadily since its inception. Unfortunately, however, the cost of production and the price of paper rose to a great height during and after the war and the former still remains very high. In order to render it possible to increase the size of the journal, the Directors of the Company installed a type-setting plant toward the end of 1921. The result has been eminently satisfactory. The printing and general appearance of the journal has improved beyond recognition. Larger issues have been published at a somewhat lowered cost, thus effecting a real economy. In many ways the Branches of the British Medical Association in Australia and the medical profession generally have been served more effectually as a result of the larger size of the issues and of the better development of the organization. It soon became apparent that the installation of one linotype machine would not suffice for a rapid augmentation of usefulness of the journal. The journal could not keep pace with the output of work of the medical profession without considerable expansion. Furthermore, the need was felt for a wide extension of the activities of the Company to serve the profession in this isolated corner of the globe. There is no printing and publishing

firm in Australia capable of handling scientific work as it should be handled. Some of this material is sent to the other side of the world; some of it appears in indifferent typographical garb; some of it fails to reach the reader altogether. There are many demands for a professional medical press which have hitherto remained unfulfilled on account of the absence of machinery. Incidentally it was found that profitable business could be secured by means of which the expansion of the journal would be rendered possible. In these circumstances the Directors of the Australasian Medical Publishing Company, Limited, at their recent meeting have determined to proceed with a scheme for the extension of the printing arrangements. All the Branches have considered this scheme and have given it their support. The members have been shown that the undertaking is a sound business proposal and they will now be asked to place at the disposal of the Company the sum of £15,000 to enable it to be carried out in its own premises. A generous rate of interest will be paid to the debenture holders. The success of the venture will depend on three things. The first is the careful and efficient organization and management of the undertaking. The second is the securing of a sufficient amount of profitable printing work to keep the machines busy. The third is the selection of expert workers in each department. The Directors have faith in the accomplishment of the first. Inquiry has elicited that there is an ample supply of work to be done and that this work can be acquired by the Company, which has established for itself a reputation for excellence in printing. Lastly, the kind of employment offered is attractive to the highly skilled worker and no doubt exists concerning the possibility of securing the services of competent men to join the present expert staff. The proposals represent the foundation of an organization planned to render effective service to the medical profession in the future. A good medical press is an invaluable asset to the profession in any part of the world. In Australia, far from the older seats of learning, the need for a complete and expansile medical press is urgent. It requires little imagination to visualize the full significance of this important step.

THE INFANT FOODS STANDARDS.

In September last a conference was held in Sydney of the executive medical officers of the several departments of health for the purpose among others of discussing the question of the standards for infants' foods. The medical officers attending this conference appear to have manifested considerable reluctance to modify the standards which had previously been adopted. Each authority recognized the necessity for uniformity, but it would almost seem as if each fought for his own ideal. The proposal to dispense altogether with a standard does not seem to have been considered. Someone with little wit and less understanding has set up the thesis that human milk contains 1.5% of proteins, 3.5% of fats and 6.5% of carbo-hydrates. On this assumption the responsible medical officers of the Commonwealth and State Health Departments have built up an artificial standard and have had the temerity of insisting on this basis for all foods offered for sale as foods for infants. While the average composition of human milk works out at figures not far removed from those of the assumed proportions, the variations are so wide and so frequent that if a standard based on these percentages were applied to human milk, the food the majority of the mothers of Australia offer their children would be regarded by the health authorities to be unsuited for infants. This question was dealt with in our issue of September 2, 1922, and the arguments put forward would seem to have influenced the conference to widen the interpretation. Formerly manufacturers of infants' food were permitted to sell their preparations, even if they varied 20% from the arbitrary standard. The conference admitted the principle enunciated that Nature is after all a better guide than men placed in administrative positions in a governmental department. It was therefore decided that a variation of 35% above or below the arbitrary percentages would be allowed. In other words the Commonwealth Health Department and the Health Departments of the six Australian States are prepared to permit manufacturers to offer for sale foods described as suitable for infants if the protein content is not less than 0.975% and not more than 2.025%, if the fat content is not less than 2.275% and not more than 4.725% and if the carbo-

hydrate content is not less than 4.225% and not more than 8.775%. Otherwise the label must indicate that the food is not to be given to infants under six months of age, except under medical direction.

There is only one real food for the infant, namely, that provided by its mother. All others are poor substitutes. Moreover, it has been shown over and over again that almost every mother can nurse her baby. There should be little need for artificial feeding, if medical practitioners and nurses managed the mothers well and if patience, perseverence and physiological principles were employed. Under certain circumstances it may be extremely unwise to permit a mother to suckle her baby. It may be that she herself is seriously affected with tuberculosis or other disease processes; her breasts may be absent or physiologically inert. Again, an orphan baby must be artificialy fed, if a wet nurse cannot be procured. In these rare circumstances some substiture for mother's milk is essential. It must further be recognized that if the mother deliberately or through ignorance weans her baby before time and starts hand-feeding, it may be impossible to restore the mammary secretion and the use of some unnatural diet cannot be avoided. While the existing state of affairs is highly undesirable and is actually detrimental to the welfare of infants, the fact has to be faced that a very large number of children receive artificial food during the period when their proper diet is mother's milk. Dr. F. H. Beare has recently published in this journal a valuable record of the methods of feeding of one thousand infants in Adelaide. He tells us that, while 85.6% are fed exclusively at the breast during the first month of life, human weakness manifests itself in the alarmingly rapid decline from this relatively satisfactory incidence. By the time the babies reach six months of age only 55.9% are receiving their rights. In the ninth month the incidence of breast feeding has dwindled to 34.1%. It should be a reproach to the medical profession that mothers are permitted to handicap their babies in this manner. The first duty of the general practitioner, therefore, is to use the most convincing arguments at his disposal in order to induce mothers to persist with Nature's method. This may mean supervision and trouble, but with knowledge and skill the large majority of mothers can be enabled to suckle their babies for from six to nine months.

What must be done when the breast supply fails or the mother refuses to persist? Cows' milk can only be recommended if its purity and freshness can be guaranteed. The milk offered for sale in our cities is to a large extent wholly unreliable. Condensed milk may be useful for brief periods, but the very high dextrose content renders it unsuitable as a stable food for the average child. That the mothers discover this is shown by the small number of babies fed on condensed milk during the first nine months of life. Then there are the dried milk preparations. "Glaxo" and "Lactogen," although far superior to condensed milk for this purpose, are given less frequently. It is interesting to note that on the basis of the standard, sweetened condensed milk fails signally on account of the very high carbohydrate content, while the protein content of both "Glaxo" and "Lactogen" does not fall within the prescribed 35% variation when one part of the dried powder is diluted with eight parts of water. What is the poor mother to do? The public health authorities inform her that she must not give these substitutes to babies under six months of age without medical advice. It is useless to expect every mother to employ a medical practitioner to guide her in the feeding of her healthy baby. If the baby is weakly, has a deficient digestion, as a result of unnecessary weaning, or reveals an idiosyncrasy in regard to the food constituents, it may be imperative to give one of the well-known and reliable infant's foods. Again the mother turns to the public health authority for permission. If she finds "Benger's Food" suitable, she is fortunate, for it happens to comply with the regulations. If she would give "Allenbury's No. 1" or "No. 2," the warning finger is pointed to 2.12% of protein, instead of the maximum 2.025%, and to 9.4% carbohydrate, instead of the maximum 8.775%. Perhaps she wishes to give "Neave's Food," but no! She is forbidden, for the protein and the carbo-hydrate are both too high, 3.7% and 11.7% respectively. She turns to "Horlick's Malted Milk," but finds the same lying statement on the label (or would do so, if the manufacturers were influenced by the Australian food regulations). This food contains but

0.86% of protein, while the minimum prescribed is 2.275%. Or, lastly, she may select "Mellin's Lacto" (that is, "Mellin's Food" mixed with pure cows' milk and reduced to powder). Here again the carbohydrate content is too high. It thus appears that only one of the foods mentioned, all of which have stood the test of time and extended trial, may be offered for sale in Australia at food suitable for infants under six months of age. We understand that in one instance at least the manufacturers are prepared to erect a large factory in Australia so that we may derive the benefit of a home supply. But the authorities are adamant. Unless the food complies with these absurd and illogical regulations, the label must bear an inscription that every doctor and every mother knows is untrue. We maintain that the adherence to a standard based on an average composition of human milk and not on a physiological estimate of an infant's requirements, is quite unnecessary and very undesirable. The health authorities should insist on pure ingredients and the publication of the formula on the label. The medical profession and the experience of mothers would suffice to distinguish between foods which are suitable for infants and those which are not. The science of dietetics is undergoing a profound modification as the knowledge of the accessory food factors increases. It is intolerable that the public health departments should appoint themselves the final arbiters as to how a baby is to be fed. The sooner the regulations are altered and the standard abandoned for a more rational guarantee of purity, the better it will be for the Australian people.

Current Comment.

EFFICIENCY TESTS.

DURING the late war it frequently became necessary to determine whether a man was physically fit or not. Sometimes it had to be decided whether he was equal to the strain attached to the performance of the duties connected with some particular branch of the service. In the later stages of the war this became more difficult as effective man-power became depleted. In this way tests for physical efficiency have of recent years received much careful study, although formerly they may have been regarded by many practitioners as of merely academic interest. Critical work has been undertaken in an endeavour to arrive at some standards of normality and to establish simple and reliable means of expressing numerically the estimated degree of efficiency. It is not only in army medical work that the necessity of adequate efficiency tests is evidenced. The physician feels the need for something definite to replace instructions given in a general way to a patient not to overdo voluntary effort. In such a case the question of fatigue, shortness of breath or palpitation is left to the patient's interpretation of subjective symptoms. The efficiency of the human body for work of any kind depends on its metabolism. If metabolism is complete and the functions of the several organs are normal and in coordination one with the other, efficiency will be greater. Neglect, on the other hand, to use such a part of the body as the muscles produces incomplete metabolic changes and impaired nutrition. If continued sufficiently long, disuse will lead to varying degrees of atrophy. Regular use and education of any organ or set of organs will, as instanced in a trained athlete, lead to increased efficiency.

The question of tests for physical efficiency has been recently investigated by Dr. G. H. Hunt and Dr. M. S. Pembrey.[1] These observers point out that the best test of the general physiological condition is found in the response to muscular work in that this form of activity calls for an adjustment of respiration, circulation and glandular function. A form of muscular exercise in daily use is most suitable for this purpose, such as walking and going up stairs. In the latter the minimum of work can be correctly calculated from the weight and the height of the ascent. They refer to the work of Benedict and Cathcart, who determined the relation between work and the combustion in the body. Benedict and Cathcart showed that the mechanical efficiency of the body (E) could be estimated, for $E = \frac{a \times 100}{b}$, the actual work measured being represented by a and the total expenditure of energy by b. Drs. Hunt and Pembrey found that the pulse gave the best indications of the effect of work, for the blood flow, the absorption of oxygen by the lungs and the discharge of carbon dioxide vary together. It was necessary to determine the condition of the subject at rest, the response occasioned by the exercise and the return to the condition at rest. The pulse rate of healthy persons at rest showed considerable variations; as a rule, the slower the pulse rate, the better the physical condition was found to be. The effect of exercise on the pulse rate gave a much better idea of a man's efficiency than the pulse at rest. The pulse rate of a man unaccustomed to physical exercise increased more and returned more slowly to its original rate than the pulse rate of a trained athlete. They found, however, that it was difficult to estimate exactly the time taken for the return of the pulse rate and that this method of calculation left out of consideration the fact that the increase in pulse rate after exercise was greater in the untrained than in the trained man. They found that a satisfactory indication could be obtained by taking the ratio between the average pulse during the two minutes following exercise and the

[1] *Guy's Hospital Reports*, October, 1921.

pulse rate at rest. This ratio, which they called the pulse ratio, they obtained by dividing the total number of beats during the two minutes immediately after the exercise by the pulse rate taken at rest before the exercise. Using this pulse ratio in relation with given standardized exercises they were able to express physical efficiency in numbers. The standard attained by a well-trained athlete was taken as 100% and others marked down from this. They further claimed that this method was easily applied and was reliable for estimating the effect of treatment and was of assistance in the diagnosis of early pulmonary tuberculosis. They promised a further contribution to the subject.

In conjunction with Mr. W. D. Hambly, the late Dr. L. E. L. Parker and Mr. E. C. Warner, Drs. Hunt and Pembrey have fulfilled this promise.[1] In the first place, they have made investigations into the range of the rate of the pulse of the same person at rest. The individual variations were found to be considerable; the largest variation for eighteen men was 36 and the corresponding figure for eighteen women was 57. The average pulse rate from four hundred and six observations on ninety-four men was 74.7, with a range of from 52 to 106; the average pulse rate calculated from one hundred and three observations on one hundred and three women was 79.5, with a range of from 53 to 116. This variation of the pulse rate at rest is considerable and must have a definite relation to the reliability of tests for efficiency in which the pulse rate is the deciding factor. No definite normal pulse rate can be laid down for a healthy person. Extensive investigations were not made into the causes of this variation, but several factors are considered to bear a possible causal relationship to it. These are the weight, athletic training, age, temperature of the body, the external temperature and the time of day. A further series of observations dealing with the pulse ratio was made. Instead of the stair-climbing test previously used, a test by stepping on to a stool a given number of times in a fixed period of time was adopted. The step was 32.5 centimetres high. The pulse ratio in this way was found to give a good indication of a man's efficiency. Observations were made on the pulse ratio of students after running a distance of one mile. Here it is interesting to note that the pulse ratio could not be taken as an indication of a man's fitness. The fittest men were those who showed the greatest percentage rise in the pulse rate and the greatest percentage fall towards the resting value. The observers claim that this confirms the low resting pulse rate of the athelete in that he has a greater range before the speed limit of the heart is reached. They make no mention of the vagus nerve in this regard. The "second wind" of the athlete must be taken into account. "Second wind" is most likely due to the cutting out of the inhibitory control of the vagus nerve.

It is interesting to note that efficiency tests have been investigated from the point of view of the cardiologist by Dr. Harold H. Brittingham and Dr. Paul D. White.[1] They point out that the effects of exercise may be measured by the change produced in the pulse rate and in the systolic and diastolic blood pressures. They found that after exercising for a definite period of time by swinging dumb-bells a fixed number of times the blood pressure of an individual rose to a maximum point about forty seconds after cessation of the exercise and fell to within six millimetres of the normal level within two minutes. If the person continued to work till he had nearly reached the limit of his capacity, the rise to the maximum point was delayed, as also was the fall to the normal level. Sometimes the "delayed summit" and "prolonged fall" were dissociated and one would be present without the other. The same features were found in patients with organic cardiac lesions and in normal persons. The conclusion at which Drs. Brittingham and White arrived was that no reliance could be placed on the test from the point of view of cardiac diagnosis or prognosis and that it was merely an indication of the patient's general efficiency.

Yet another effort has been made to correlate different types of efficiency tests. Mr. W. Finkelstein and Dr. Jesse F. Williams have investigated the Schneider and the Sargent tests.[2] The Schneider test determines the pulse rate reclining, its increase on exercise and its return to the normal after exercise. The Sargent test consists of an endeavour to test strength and energy by determining how far a person can project his body in an upward direction from a crouching position. The index is obtained by multiplying the weight of the person by the height of the jump and dividing the result by the height of the person. The correlation between the results of these two tests was very close, but not perfect. From the fact of this high correlation the inference is made that cardiac efficiency is a good indication of general efficiency. They also conclude that the Schneider test requires revision in order to allow for the great variation existing in normal pulse rates.

In discussing this subject it will be seen at once that no rule of thumb method can be adopted. The human body is not a machine composed of so many spare parts assembled in definite order. There is no standard of normality to which an individual can conform, nor is a normal efficient person a fixed quantity. The investigation of a question of this sort demonstrates quite clearly the futility of the hard and fast lines so frequently laid down in the examination of candidates for admission to any of the public services or of proponents for life insurance. Much more work in this subject, with an endeavour to correlate the results obtained, is necessary in the first place to determine with some degree of accuracy the limits between which normality can be claimed. Even when this is done, it will always be a question of the examination of a person as a variable quantity and as an individual rather than as a unit of a group.

[1] *Guy's Hospital Reports*, October, 1922.

[1] *The Journal of the American Medical Association*, December 2, 1922.

[2] *The Journal of the American Medical Association*, May 13, 1922.

Abstracts from Current Medical Literature.

OPHTHALMOLOGY.

Correction of Squint by Muscle Recession.

P. C. JAMESON (*Archives of Ophthalmology*, September, 1922) describes a new operation for convergent squint which he has performed fifty-six times in thirty-nine patients. The operation consists of a tenotomy with fixation of the tendon by sutures to the sclera behind its original insertion. The internal rectus is freely exposed by a curved conjunctival incision and the flaps are undermined in a backward and forward direction. The muscle is then undermined and completely separated from the globe and its capsular continuity is severed above and below by incisions carried back beyond the equator. The muscle is grasped by a Reese forceps and severed from its insertion. The position behind the insertion where the tendon is to be attached is measured in millimetres and noted. Three single-armed sutures are provided. The central suture is introduced from without the conjunctival lip and made to perforate the outer surface of the muscle. To insure a good grip the needle is put through the muscle again from the under side, just above its centre, and is brought out in the upper side of the muscle, the thread being looped round that of the first perforation. The suture is now continued right on through the sclera and finally through the lip of the conjunctiva nearest the cornea. The two remaining sutures are inserted about three millimetres above and below the centre and are carried directly through the conjunctival lip, muscle end, sclera and conjunctival lip nearest the cornea. The sutures are then tied, holding the muscle to its new scleral position and closing the conjunctival opening. They are removed on the sixth or seventh day. The operation may be performed on one or both eyes. The author found that a single recession of five millimetres with scleral fixation would correct a deviation of 25° in a high percentage of cases or a double recession of four millimetres in both sides a deviation of 35° or more.

The Diagnostic Use of the Uveal Pigment in Injuries of the Uveal Tract.

A. C. WOODS (*Archives of Ophthalmology*, September, 1922) has examined the sera of seventeen patients suffering from intra-ocular injury involving the uveal tract by means of the complement fixation reaction with an antigen of uveal pigment. One group comprised ten instances which gave a positive reaction and which healed up without any sympathetic disturbance. Three eyes were excised for various reasons. In the second group the sera of three patients did not yield a reaction and at the same time showed signs of sympathetic disturbance. In the third group, three patients showed alarming symptoms in the injured eye, but no manifestations of sympathetic disturbance in the second eye; the sera of all these patients failed to yield a reaction. The author points out that a consideration of these results shows that a positive reaction was always followed in time by a subsidence of inflammation without any manifestation of sympathetic disturbance. Failure to react is not so conclusive. The serum does not yield a positive reaction before ten days after injury and sometimes not before four weeks. Further, the power of the serum to react may be followed by a period in which no reaction is obtained and this may again give place to a power to react. However, if at the end of six weeks the serum of a patient does not yield a positive reaction, it seems unlikely that it will ever do so and in such instances the possibility of sympathetic disturbance should be carefully considered.

The Naso-Lachrymal Passages.

D. M. CAMPBELL, J. M. CARTER AND H. P. DOUT (*Archives of Ophthalmology*, September, 1922) have made an X-ray study of the lachrymal passages of sixty patients. Some of these were normal, some were obstructed and some had been operated upon. The investigation was undertaken with the object of obtaining information that would be useful in determining the operative treatment. The passages were injected with oil and bismuth and skiagrams were taken in lateral and postero-anterior positions. The results showed gross variation in the appearance and size of the structures. The authors show that the two canaliculi, the sac and duct may present a most bizarre appearance and may be malformed and very grotesque in contour. The passage of a probe in these circumstances may easily do considerable harm. It may happen that suppuration still continues after operation for removal of the sac, showing that part of the sac still remains. The skiagram may then show the possibility of doing a short-circuiting operation or it may be the means of localizing the remaining portion of the sac, so that it can be removed. The skiagram also demonstrates the position which the lachrymal duct occupies in relation to the middle turbinate bone. The authors favour the nasal drainage operation and employ the nasal route.

Sub-Conjunctival Cataract Extraction.

ANGUS MACGILLIVRAY (*British Journal of Ophthalmology*, August, 1922) has adopted the sub-conjunctival method of extracting cataract and gives an account of his experience with some three hundred instances. The cornea is punctured one millimetre behind the limbus and one millimetre above the horizontal diameter and the counter puncture is made in the corresponding position. The scleral incision is carried upwards and completed in the ordinary way, but without dividing the conjunctival bridge. At this stage the edge of the knife is directed backwards and by one or two sawing movements a conjunctival bridge is carried upwards for twelve millimetres, the bridge being generally ten millimetres in breadth. No iridectomy is performed. After capsulotomy the posterior lip of the wound is depressed under the conjunctival bridge with a curette and by applying steady pressure over the ciliary region from below upwards, the lens, as a rule, passes without difficulty through the pupil into the anterior chamber. Thence it passes upwards through the scleral wound under the conjunctiva and so on to the surface. Cortical débris is washed out and the iris replaced, if necessary. There is little tendency to iris prolapse. The author would not revert to the old, open, combined operation and recommends this method as the best known at present.

Furunculosis and Conjunctivitis.

A. BERNARD (*Le Journal des Sciences Médicales de Lille*, September 24, 1922) relates the history of a young woman of twenty-five years who had suffered for fifteen years from boils on all parts of the body, together with attacks of phlyctenular conjunctivitis. An autogenous vaccine of staphylococci was prepared and injected in increasing doses during two months. The doses ranged from one hundred million to two milliard organisms. For two years she has had neither boils nor conjunctivitis.

LARYNGOLOGY AND OTOLOGY.

The Optic and Vidian Nerves and Sphenoidal Sinus.

GAVIN YOUNG (*Journal of Laryngology and Otology*, December, 1922) has made dissections of thirty specimens of the sphenoidal sinuses and adnexa of cadavers not specially selected. In eight instances (26%) the nasal accessory sinuses were healthy, pus was found in one or other of both maxillary antra in thirteen instances (43%), while one or both sphenoidal sinuses contained pus in seventeen instances (56%). Normally the posterior ethmoid cell lies on the antero-lateral aspect of the sphenoidal sinus. Many variations were found and these were largely due to the amount of bone absorption when the cells were in process of formation. In the sclerotic type the posterior ethmoidal cell lies laterally to the sphenoidal sinus and in this type the optic nerve, which lies in close relation to the postero-lateral angle of the post-nasal sinuses, is related, not to the sphenoidal sinus at all, but to the posterior ethmoidal cell. The relation changes according to the amount of bone absorption until in the thin-walled sinuses the canal of the nerve is in relation to the sphenoidal sinus posterior to the posterior ethmoidal cell. In twenty-seven instances (90%) the bone between the nerve and the sphenoidal sinus was so thin in part as to be practically transparent and in four specimens (13%) there was a hiatus in the bone covering the nerve. In many instances the inter-sphenoidal septum was definitely deflected from the middle line. This resulted in some specimens in the

sphenoidal sinus of one side being in relation to the hetero-lateral cavernous sinus in thirteen instances (46%) and to the hetero-lateral optic nerve in nine instances (30%). The sclerotic type only occurred in 10% of the specimens. In the thin-walled type a striking feature was the bulging down from the roof of the sinus of the fossa containing the pituitary body—a bulge of usually five to eight millimetres of transparent, easily-injured bone. In seven instances (23%) the walls of the optic nerve canal bulged into the sinus and foramina in the lateral ethmoidal and sphenoidal walls for the transmission of blood vessels to and from the orbit were numerous. One or both sphenoidal sinuses were loculated by incomplete but well-formed septa in 53%. The Vidian nerve, which runs forward under the floor of the sphenoid sinus to supply the sphenopalatine ganglion, was found to be separated from the sinus in 50% by transparent bone and in three instances (10%) there was a hiatus in the roof. Neuritis of the Vidian nerve may cause otalgia, the nerve being connected by the great superficial petrosal nerve with the geniculate ganglion of the facial nerve, which in turn is connected by two branches with the auditory nerve. It is suggested that such a neuritis might be the origin of some of the obscure conditions presenting the Menière's syndrome.

Scarlet Fever Otitis.

W. T. GARDINER (*Journal of Laryngology and Otology*, October, 1922) investigated the incidence of ear complications, their mode of onset *et cetera* in patients suffering from scarlet fever at the Edinburgh City Hospital for Infectious Diseases. From a total admission of 3,728 patients he found in the 1920-1921 period the percentage of ear complications to be ten. In the 1921-1922 period the percentage was six. He found that otorrhœa commenced most frequently during the first three weeks of illness, with a definite inclination in the direction of the first week. Otalgia is a reliable though not an infallible sign of ear mischief. Discharge occurred twice as often without pain as with it. Pain was usually present in the first and second week, but seldom after that time. With pain there was usually a rise in temperature. The discharge was purulent in the majority of instances. The author holds a foul odour to be suspicious of diphtheria when there is no previous history of discharge. In the majority of instances there was desquamated epithelium in the meatus. The author has not seen the tympanic membrane destroyed in a few hours or even in a few weeks. A previous history of discharge was ascertainable when a larger perforation was discovered. The perforation was usually found in the anterior portion of the drum. The painless onset of otorrhœa at any stage of scarlet fever he held to be diagnostic of adenoids. Adenoids were present in 47% of patients with otorrhœa. When adenoids are present in any patient suffering from scarlet fever, they should be removed before the child leaves hospital; the tonsils, as a rule, should be removed at the same time. The operation should be preceded by a prophylactic dose of five hundred units of diphtheria antitoxin. Otorrhœa on an average dried up fourteen days after operation. The drumhead ruptured so easily and readily in children that the author refuses to be dogmatic regarding early paracentesis. Of three hundred patients with otorrhœa twenty-one (7%) developed mastoid complications. These were of two types. The first type developed in the third week with pain, tenderness, œdema and rise of temperature to 38.4° C. (101° F.). A small amount of pus was usually found in the antrum and the whole of the mastoid process was inflamed. The second type developed in the fourth or fifth week, usually without pain or tenderness, until the sub-periosteal abscess appeared. As a rule, there was no pyrexia. In these instances the mastoid process was an abscess cavity containing pus and small sequestra, the sinus wall was often found bathed in pus and in practically every case streptococci in pure culture were found. In no instance was the radical or modified radical operation necessary.

Fugacious Œdema of Mastoid Region with Paroxysmal Hæmoglobinuria.

C. J. KOENIG (*New York Medical Journal and Medical Record*, July 19, 1922) reports the history of a boy, aged eight and a half years, who, after he had been ice-skating, became feverish, with pain in the right ear and throat and slight hyperæmia of the drum membrane. He had had scarlet fever ten months previously. Hot, moist applications were ordered. Two days later an extensive œdema was found involving the parietal, mastoid, temporal and sub-occipital areas of the right side. This persisted for some hours. The pulse was arrhythmic. Analysis of the urine twenty-four hours later showed at first chloride retention, albuminuria, hæmoglobinuria and an abundance of indol and skatol. The child had had no motion for three days. The author thinks that the indol and skatol formed from bowel putrefaction and under the influence of the cold had been absorbed into the blood, causing a hæmoclasic and colloidoclasic shock, producing symptoms dependent on irritation of the organo-vegetative system, *viz.*, hæmoglobinuria, vagotonia and œdema. The rise in temperature was due to a concomitant middle ear infection, as the shock caused by skatol and indol has been shown by experiment not to be accompanied by fever. Further, it has been shown that, if there be any excitation of the vasomotor or sensory nervous system locally, an œdematous infiltration may appear in that region. The hot applications were probably the irritant in this case. The vaso-hæmatic symptoms usually precede the hæmolytic. Syphilis is said to be a real and necessary factor in producing such a picture and there was reason to believe that the patient was the subject of hereditary syphilis.

Congenital Bilateral Occlusion of the Choanæ.

A. J. M. WRIGHT (*Journal of Laryngology and Otology*, November, 1922) records the histories of two sisters, aged fourteen and fifteen, who complained of "stuffy noses." In each instance there had been no tendency to asphyxia at birth, but at that time and in infancy the breathing had been noisy. From the age of two years the respiration had become quiet. The children had had alleged adenoids removed, the once twice, the other thrice, for nasal obstruction. There were three other children in the family, aged twelve, eight and six, who all breathed normally. Both the patients were well developed and intelligent, there was no chest deformity, the palates were moderately high, the nasal passages were wide, the septa straight and the turbinates small and crenated in appearance. There was no sense of smell. The posterior nares were found completely occluded by bony plates. They were removed by chisel and mallet, as was also the posterior twelve millimetres of the nasal septum. The sense of smell partially returned after operation in one patient and there was a considerable degree of nasal breathing established in both. The author points out that the signs found contrast with those occurring in patients whose adenoids have been neglected and in whom chest deformity appears together with narrow and highly arched palates, crowded and carious teeth, collapsed *alæ nasi*, narrow nasal passages, deflected septa and enlarged turbinates. The ears also present evidence of infection. These symptoms are the result of respiration through a partially obstructed airway or of infection or both.

The Ventricle-Stripping Operation.

MICHAEL VLASTO (*Journal of Laryngology and Otology*, December, 1921) concludes from the consideration of many dissections of larynges of the horse and man, as well as of operations carried out in the *post mortem* room, that stripping the ventricle of the larynx in order to produce raw surfaces, the union and cicatrization of which would draw the vocal cords aside and thereby increase the *rima glottidis*, finds no application—theoretically at any rate—in man. The ventricle and saccule in the horse differ anatomically from those in man. In the latter the ventricle is a shallow sac slightly undermining the false vocal cord, but not the true. Under cover of the most anterior part of the false cord is the aperture of the mouth of the saccule. The saccule is a small diverticulum of the ventricle which passes upward in no relation whatsoever to the true vocal cord. It is closely invested by muscle sheaths (thyreo-epiglottideæ and thyreo-arytenoid) and would be very difficult to evert. In the horse the ventricle and saccule form one large sac which below clothes the outer side of the true vocal cord, the attachments are extremely loose and the whole structure can be everted with ease.

British Medical Association News.

SCIENTIFIC.

A MEETING OF THE NEW SOUTH WALES BRANCH OF THE BRITISH MEDICAL ASSOCIATION was held at the B.M.A. Buildings, 30-34, Elizabeth Street, Sydney, on January 12, 1923, DR. T. W. LIPSCOMB, the President, in the chair.

Puerperal Sepsis.

DR. T. W. LIPSCOMB read a paper entitled "Puerperal Sepsis" (see page 169).

MR. FOURNESS BARRINGTON expressed his admiration of the President's concise, clear and clinical account of puerperal sepsis. In no disease was prophylaxis more important. He thought that prematernity care should be extended to the patients just before term, especially in primiparæ, so that the relative proportion of the head to the pelvis could be observed. The post-mature fœtus was a cause of dystocia and indirectly of sepsis. The most important prophylactic measures included stricter asepsis and antisepsis, for the vulva was the most septic area in the body. It could not be sterilized; it could only be made less septic. Fewer or no vaginal examinations should be made during labour; when they were made, the most careful disinfection of the vulva and neighbouring parts, the hands and gloves was most important. Conservatism and non-interference during normal labour was important; the progress of most normal cases could be conducted by abdominal palpation. Manipulation should be gentle if interference was necessary and, when forceps were needed, a reliable axis-traction pattern should be used. Such forceps should be mechanically correct if they were to do their work to the best advantage when put to the test. Most important of all was careful conduct of the third stage, for a well-retracted and empty uterus was the best safeguard against intra-uterine sepsis. He advocated daily massage of the uterus by a reliable nurse as part of the morning toilet. It kept the uterus contracted and retracted and prevented accumulation of blood clots, which occasionally caused signs of sapræmia. Letting the patient sit up early favoured drainage. As regards actual treatment, any rise of pulse or temperature needed a most careful search for a cause. If an extra-genital cause could not be found, the case should be regarded as one of puerperal sepsis. In all cases of puerperal sepsis, early diagnosis was imperative and a bacteriological examination should be made. The cervix should be exposed and swabbings obtained from the interior of the uterus. From these immediate smear preparations and cultures should be made. The advisability of exploration of the uterus was a question for nice decision. As a routine, with either finger or curette, it should be condemned. It should be done with the fingers when there was a reasonable suspicion of retained secundines—a large, tender uterus, pointing to delayed involution and fœtid lochia. The uterus should be emptied with the gloved index and middle fingers, the fundus being well pressed down by the left hand. A curette should not be used if it could possibly be avoided. If there was a piece of decidua or placenta too adherent to remove with the fingers, it should be raked away with a blunt curette of ample size. The curette should be regarded as a rake and not as a scraper, for what was in the uterus came away easily. Scraping away septic material from the uterus with a curette as a routine procedure was most dangerous. It ploughed up the ground, scattered the seeds of sepsis into freshly opened veins and lymphatics and removed the leucocyte bank. When the uterus was completely emptied by the fingers, it should be swabbed out with gauze squares soaked in biniodide solution, one in two thousand, dried out with iodoform gauze and finally the whole cavity loosely packed with clean iodoform gauze.

If the uterus were empty and well involuted and the secretions peculiar to the puerperium—the milk and lochia—had dried up, it should be left alone. There could be no doubt of this; the less the uterus was interfered with in severe cases of infection, the better for the patient.

In generalized blood infections, septicæmia and pyæmia, the outlook was extremely bad. The final result depended on the virulence of the organisms and the resisting powers of the patient. He had found intravenous injections of isotonic "Eusol" curative in some cases of pyæmia. Puerperal blood infections were like cases of enteric fever. The sponge should never be thrown in, for some patients recovered after the severest illness.

In localized pelvic infections with an acute inflammatory mass, the best treatment was to leave the patients alone. Many got quite well with this conservative neglect. Pus should be let out when it formed low down in the pelvis by posterior colpotomy and in the broad ligament where it pointed or by a grid-iron incision in the groin.

DR. CONSTANCE D'ARCY thanked the President for his address. It was instructive and gave much food for thought. There were several points on which she did not agree with him. She did not think that he had intended it to be understood that the uterus should be cleaned out in every case. She thought that it would be dangerous for obstetrics generally if it were the normal practice to explore and curette the uterus, except in such instances as Dr. Barrington had pointed out. She had seen many examples of both types of infection. After many years of the old practice of curettage and using intra-uterine douches she was satisfied that the results then were worse than at the present time. The majority of obstetricians were more conservative and only used the curette when the history suggested retained secundines. Dr. MacCulloch had recently delivered an address to obstetric nurses and he had laid stress on the point that the death rate from puerperal sepsis was no better than when he was a young graduate. Dr. D'Arcy maintained that such a condition of affairs, when it was found that no improvement had been made, left practitioners in a position of doubt. Obstetricians at the present time dared more than formerly, particularly in regard to the use of forceps. Much more was dared under strict aseptic technique, for there the practitioner was surrounded by the usual methods appertaining to the operating theatre. It was a bold thing to say, but she believed that the present system of small nursing homes was a curse to the country. The same treatment and the same conveniences were not available as in a public hospital, where patients were herded together. Nothing less good should be offered to private patients. Even with the best intentions, many nurses failed to realize the importance of complete asepsis. No sterilization under pressure was available and the linen was frequently used just as it was returned from the laundry. Often, perhaps, the cat had sat upon the clothes basket. Any risk of infection should be eliminated. Here was a great public need to be met. At the present time, when domestic help was impossible, women should get safe midwifery. Arrangements should be made for some form of hospitalization or private wards where women could go under any reputable practitioner they cared to choose and have the treatment they deserved. This pertained in America. The whole subject bristled with difficulties, but these could be overcome. It would not be detrimental to obstetric nurses generally, for these would have to be employed under such a scheme as they were in private nursing homes at the present time. In conclusion, Dr. D'Arcy referred to the use of polyvalent serum. Better results had been obtained at the Royal Hospital for Women at Paddington since serum from the Commonwealth Serum Laboratories had been used. This was probably due to the fact that local strains of streptococci were used in its preparation. It was advisable to use a large initial dose and smaller subsequent doses.

DR. W. T. CHENHALL said that, having read the original paper on puerperal infection which had initiated the present important discussion, he felt that he should stand exempt from the present discussion. His statements had been severely criticized in an informal manner by many colleagues, but he welcomed criticism of constructive character, since his desire was for the general good of the profession.

The President's opening address was full of practical wisdom and formed an excellent precept for those younger men who were following in the path of general practice in which each and all of the problems of puerperal infection would be met.

Dr. Chenhall felt bound to reiterate his protest against the use of the curette. He regarded it as a dangerous instrument, even in experienced hands, and utterly failed

to understand how its service could avail when blood or lymphatic infection had already occurred. Nature endeavoured to establish a protective zone. The curette—and especially the sharp curette—could scarcely fail to disturb Nature's barrier.

Dr. Barrington had very carefully set before his colleagues an excellent and distinctly graphic picture of the two types of uterus found in puerperal infection. In the absence of evidence of blood infection, it would be quite justifiable to empty the large, soft, flabby uterus of its decomposing decidua, but not by means of a curette. That was a justifiable procedure which could be satisfactorily conducted by other means well known to members. In the case of the small uterus, presumably empty and with strong suspicion or distinct evidence of bacteriæmia, it seemed absurd to curette the uterus. There were many debatable problems in this extensive subject and he had attended the meeting, not with the intention of speaking, since his views were already known, but of being a patient listener, desirous of hearing the views of colleagues and especially of the members from the provinces who were attending the post-graduate course.

DR. R. I. FURBER, D.S.O., applauded Dr. D'Arcy's remarks. He hoped the time would come when no woman would be delivered in any other surroundings than those of a modern operating theatre. In American hospitals the results obtained were as close to perfection as possible. In Australian hospitals with adequate equipment, equally satisfactory results could be obtained. Dr. Furber referred to two points. The method adopted with the ante- or intra-partum sitz bath or plunge bath was unsatisfactory. When labour started, the nurse usually gave the patient an enema and put her into a hot bath. In this way a solution of skin débris, fæculent material and many micro-organisms were washed into the vagina. This was the case particularly in patients with patulous vaginæ. In primiparæ this was not so noticeable. It was possible that the ante-partum bath might be the cause of some otherwise unexplained infections. Another matter of importance was the question of coitus. Instances had been recorded in which coitus had ruptured the membranes. It was always necessary to issue a warning against coitus in the later stages of pregnancy. In regard to the question of active interference in puerperal sepsis he could not altogether agree with the President. Instrumental or finger curettage was not advisable, except when indicated by hæmorrhage. In regard to the statements that the most virulent infections were due to the streptococcus or the *Bacillus coli communis*, it was well known that the staphylococcus, when it became a septicæmic organism, produced an affection as severe as any other. In regard to treatment, Dr. Furber had not seen any beneficial results from the use of vaccines. He thought, however, that the use of serum was helpful and that serum prepared from local strains was better than the imported article. A material advance had been made in obstetrics at the Medical School. This was largely owing to the unwavering perseverance with which Dr. Barrington had forwarded the cause of obstetrics. Dr. Windeyer was carrying on the good work and was trying hard to improve the standard. Tutorships in obstetrics had been established at each of the three teaching hospitals. This was an advance, for it meant that students received some clinical teaching before their period of residence.

DR. J. MORTON said that he wished to emphasize chiefly the importance of early treatment in puerperal infection. Before doing so, he wished to refer briefly to the discussion mentioned by the President and reported in *The British Medical Journal* of August 21, 1920. This represented the views held at that time by many prominent gynæcologists in England.

Mr. Victor Bonney, in his opening address, had brought out the point that infection from extrinsic sources, which had formerly caused epidemics of puerperal sepsis, was at the present day rare owing to the general adoption of aseptic and antiseptic methods. With great reason he had shown that nearly all cases of infection were autogenous, that is, from inoculation with intestinal organisms which were always to be found about the vulva and perineum. He (Dr. Morton) was quite in accord with this and wished to say that in his experience women who for various reasons had been delivered in the isolation block of the hospital to which he was attached, did just as well as those delivered in the obstetric theatre. This was sufficient proof to his mind that with ordinary precautions infection need not be carried from one patient to another.

Bonney had said that while the most obvious way germs gained entrance into the uterus was on the hands or instrument, in by far the larger number of instances no such introduction had taken place, the most that could have happened being implantation into the cervix or vagina. In many instances the delivery had been normal, whilst in many no intra-vaginal examination had been made at all, the delivery having occurred before the arrival of the attendant. He stated that *post mortem* examination invariably indicated that the infection of the tissues had begun at the placental side and since, as a rule, the placental site was situated in the upper uterine segment, the infection most commonly spread along the ovarian veins. On this account in intractable cases and more especially when a swelling could be detected on palpation of the ovarian region, he had advocated ligature or excision of the ovarian leash of vessels with their accompanying lymphatics and he had claimed success in a number of cases.

Dr. Morton pointed out that Beckwith Whitehouse, arguing on the analogy with war wounds and experience gained during the war, had adopted Carrel's method of continuous irrigation with Dakin's solution after the uterus had been carefully curetted to remove débris and after hæmorrhage had been stopped, if necessary, with a temporary uterine pack. He had also claimed success with intravenous injections of "Flavine" solution in strengths of one in one thousand or of normal saline solution by injecting ten to fifteen cubic centimetres into the median basilic vein once, or in severe cases twice a day.

H. Leith Murray had dealt with sera and vaccines. He had stated that organisms had been grown from the blood of 46.4% of one hundred and ninety-six patients. Of these infections 93% were streptococcal in origin. He had advised large doses of antistreptococcal serum without waiting for blood culture, fifty cubic centimetres being given intravenously and diluted with not less than equal amounts of normal saline solution. If no improvement resulted in twelve to twenty-four hours, the inoculation should be repeated, the brand of a different maker being used, if possible.

In the discussion at the Royal Society of Medicine most speakers apparently had more faith in prevention than cure. Laidley Holland had advocated rectal examination and had said that routine vaginal examination should be abolished. He had employed the rectal route for eight years and could feel the state of cervix, the sutures and fontanelle, the degree of descent of the head and even a prolapsed cord with the utmost clearness. Armand Routh had said that most cases of infection could be prevented by douching with strong iodine solution if a finger or instrument had been passed into the uterus. If the temperature rose next day he had advised the induction of anæsthesia and digital exploration, followed by the application of a 25% solution of iodine to the interior of the uterus. As to curetting, some speakers had opposed it and most had recommended extreme care and the use of the finger or fingers rather than a curette; if a curette had to be employed, it should be a large, blunt one.

Dr. Morton pointed out that in all the discussion it was remarkable that there had been no mention of the time element. While there had been frequent reference to the application of general surgical principles to puerperal infection, the time factor seemed to have been quite ignored. If there was one surgical principle more important than another in the combating of sepsis, it was the principle of early attack. It was the application of this principle in the war that had led to the establishment of skilled surgical groups close to the fighting line. It was realized that the earlier the wounded were dealt with, the better the results would be. Nothing counted so much as time. The late John B. Murphy, of Chicago, had said that delay was the cause of the greatest mortality in surgery. There were few who did not realize from sad experience the truth of this statement and yet in puerperal infection there was often failure to apply it. The wait-and-see policy seemed to be ingrained in men and by the time they realized that the uterus might be infected, it

was often too late for effective treatment. It was usually the third or fourth day before any serious steps were taken and by that time the infection might have got into the placental sinuses. Dr. Morton felt sure that this was all wrong and that speeding up was necessary if results were to be improved. The only safe course was to assume that the very first rise of pulse and temperature was of uterine origin and to act at once. The simplest and most effective way of doing this was to insert a broad strip of gauze saturated with tincture of iodine to the top of the uterine cavity and to leave it there for about twelve hours. While this should be packed lightly at the top of the cavity, the lower part should be left in a single, straight strip, so as not to occlude the cervix. The gauze strip did not interfere with drainage, which, next to quickness of attack, was the greatest factor in dealing with infection.

A good deal of sepsis was due to lochial stasis, even when the discharge was apparently sufficient. This could often be demonstrated by gently opening the cervical canal with forceps after cleaning out the vagina. If patients were allowed to sit up and empty the bladder or bowels in the natural way, there would be less liability to stagnation of lochia.

In regard to douching after intra-uterine manipulation, there was a practical point which Dr. Morton had not seen written down and which it was important to remember. The douche should be given before the placenta separated, because then the uterine cavity was lined by a smooth epithelial surface like a glazed tile and was easily cleaned. This should be compared with the roughened and bleeding surface after its separation.

In all vaginal or intra-uterine manipulation he firmly believed in the free use of liquid soap. It was the best and cleanest lubricant; it facilitated the passage of the hand and the fœtus and caused no irritation.

If they were to be aseptic as to the hands, gloves should be worn. In midwifery the hands could not be trusted. This applied to the nurse as well as the doctor, perhaps more so, because she had more to do with the bowel.

With regard to forceps, Dr. Morton thought that it was certain that a large amount of damage and sepsis was due to impatience and hasty delivery with forceps; damage—immediate and remote—to the child as well as to the mother. He thought it would be better if midwifery forceps were cast out and forgotten altogether. The trouble was that the patient clamoured for an early release from her sufferings and deliverance was too often sought in the forceps. Here, again, the great factor, time, was disregarded. Delivery in haste meant repentance at leisure. The greatest friend he had possessed, when in general practice, was a chloroform apparatus for the patient's own use, which every medical man doing obstetric practice should possess. With it the patient could keep herself oblivious to her sufferings without risk and was invariably grateful afterwards, even though she might appear to be in pain during the confinement. He looked upon "Pituitrin" as the substitute for forceps. He had seen nothing harmful and many benefits from its use. If time and "Pituitrin" would not effect delivery, the case was one for other measures than forceps.

DR. RALPH WORRALL said he would try to avoid repetition of what others had said, including compliments to the reader of the paper, which, however, were deserved. He thought he heard the author say: "The majority of cases of puerperal infection were due to auto-infection." That might be a comforting doctrine and one useful in combating the suspicions of the patient's husband and friends, but he (Dr. Worrall) held it to be most dangerous, notwithstanding that it was put forward by Victor Bonney in the recent debate on the subject in England. It was opposed to general experience. In spite of all that had been said of late to the contrary, it was incontrovertible that the more careful and elaborate aseptic precautions, the less would be morbidity and mortality. The principle of aseptic technique, that the field of operation should be rendered as aseptic as possible, must, of course, be observed and in this connexion he would like to say that this could be carried out much more effectively with the patient in the dorsal position. He favoured delivery in the dorsal position, whether naturally or by application of forceps or version.

Next to elaborate aseptic precautions before, during and after delivery, he would place patience in the third stage and then the pre-maternity care which had been referred to. He agreed with Dr. Furber that coitus should be barred during at least the last month. Women should also be warned against examining themselves. He had met with a case of infection arising apparently from this cause and resulting in suppurative peritonitis, fortunately localized, for which he had operated with success. He had reported the case to the Branch some years ago. When infection had taken place, it was his experience as a consultant that the practitioner usually curetted and gave serum. He was not sure about the serum; to-night's debate would give encouragement in its use, but he thought, on the whole, curettage was highly mischievous. He said that, but still he favoured curettage in suitable cases and had curetted numbers of women for sapræmia or septic intoxication with most gratifying results. He had been asked how he differentiated between the cases suitable and those not suitable for curettage and he had replied, first of all by studying the history of the case; had there been any difficulty with the third stage or excessive hæmorrhage? Secondly, he considered the character of the discharge. This was profuse, offensive in septic intoxication and scanty or not abnormal in septic infection. Thirdly, he noted the degree to which involution was arrested; always most in septic intoxication. He measured the height of the fundus from the fixed point, the pubes, daily with a stationer's brass foot measure rule, which could be boiled. The fundus was about 12.5 centimetres (five inches) immediately after delivery, but in a short time, when the sinuses filled up with clot, it was about 13.75 centimetres (five and a half inches). After the second day it should fall one and a quarter to two centimetres each day and reach the pubes on the tenth day. These measurements, recorded on the chart, were worth carrying out and constituted a guide to the prognosis and treatment.

If signs and symptoms pointed to dead tissue in the uterine cavity, should there be manual exploration or curettage? He said, unhestitatingly, curettage. In manual exploration the hand was forced into the vagina, two or three fingers into the uterus and the fundus forcibly depressed to enable the fingers to palpate the entire interior. It was evident that the dangers from traumatism and displacement of septic thrombi were far greater than in curettage. He used a large, sharp curette very gently, but went over the whole surface in a systematic manner. The cavity was then thoroughly flushed out with normal saline solution. Finally iodine—the great value of which he had called attention to years ago—should be used in the following manner. A roll seven and a half centimetres (three inches) wide of iodoform gauze was placed in a boiled breakfast cup and covered with a couple of ounces of tincture of iodine, the cup was placed against the vaginal retractor and the gauze passed right up to the fundus and into each cornu. If the gauze were not carried up to the fundus, it acted as a cork and dammed back the secretions. Used in the proper manner, it benefited as an innocuous antiseptic, as a powerful promoter of uterine contraction and, thirdly, as a drain. It should be removed in twenty-four hours and no further local treatment carried out. As a rule, in the treatment of puerperal infection reliance should be placed upon general principles. Nature should be aided by food, as much fresh air as possible without a draught and sponging *et cetera* should be insisted upon. A mixture of ergot, strychnine and digitalis should be given, to promote uterine contraction and raise blood pressure. Quinine hydrochloride, 0.6 gramme (ten grains), dissolved in four cubic centimetres (one fluid drachm) of brandy in five hundred and sixty-eight cubic centimetres (a pint) of saline solution should be given every six hours *per rectum*. This also tended to contract the uterus, lowered the temperature and diluted the toxins. Drainage would be favoured by propping the patient up in a semi-Fowler position. Finally, it should be remembered that puerperal infection resembled all other surgical infections in that much could be done to prevent, but little to cure.

DR. A. J. GIBSON expressed his appreciation of the President's paper. Undoubtedly the most important point was that of pre-natal examination. It was necessary to have a proper appreciation of the conditions underlying the

delivery of the patient. In regard to sepsis, it was his experience that the patients could be divided into two groups. The first group comprised those with retained secundines and the second group those suffering from the acute fulminating type of the disease. Treatment in the latter type was very disappointing and it was difficult to trace the source of the infection. He recalled three cases which showed the extremely acute nature of the infection. The first patient had done well and was to have got up on the tenth day. On this day, however, she had suddenly developed acute septicæmia and had died in three days. The cause of the infection had been obscure. The second patient had been attended by the district nurses from the Women's Hospital, Crown Street. She had delivered herself. At this time an epidemic of scarlet fever had been prevalent. The patient had developed a scarlatiniform rash. She had been acutely ill and had had a septic throat. A diagnosis of scarlet fever had been made and the patient had been sent to the Coast Hospital. The acute nature of this infection had been evidenced by the fact that, although he had worn gloves, gown and mask while examining the patient, he had contracted scarlet fever. He had attended no other patient with scarlet fever at that time. The third patient had delivered herself. He had been called to see her on account of the supervention of maniacal symptoms. As far as he had been able to judge, no vaginal examination of the patient had been made, yet she had developed an acute septic condition which had been either scarlet fever or septicæmia. Dr. Gibson stated that two days after examining the patient he had become ill with nasopharangeal infection and had been laid up for five weeks. Of the patient's two children, one had developed scarlet fever and the other had been infected in the same manner as himself. The nurse attending the patient had become acutely ill with what had been diagnosed as influenza and the doctor who had attended the nurse had been similarly affected. The patient had died two days after admission to the Coast Hospital. In cases such as these treatment was of little avail. In the majority of patients, however, there was some retained débris and the most important indication in dealing with these patients was the pulse rate. A rising pulse rate was always to be regarded as a danger signal. It was most important to exclude the presence of other diseases and to explore thoroughly the uterus. The uterus should be explored with the finger. As much of the offending material as possible should be removed in this way and then a large, sharp curette should be very lightly used. Gentleness was most important. As Dr. Barrington had said, the curette should be used as a rake. All the material could not be removed by the finger. The finger would give an idea of the amount of débris present, but by using the curette alone some material was apt to be left behind. The two methods should be combined in this way. After a free use of the douche iodoform gauze should be passed up to the fundus. If the uterus was clean, the gauze would be clean on removal. The uterus should be packed with iodoform gauze and iodine and this packing should be allowed to remain for eight or ten hours. Exploration of the uterus should be made on the first indication of trouble. If this were done, the patient's temperature would fall almost by crisis. Early treatment would lead to success. Exploration in the late stages only added fuel to the fire and caused infection of the broad ligaments, pyæmia and so forth.

DR. H. C. E. DONOVAN wished to thank the President and the other speakers for their contributions to the discussion. Much had been said about prophylaxis and the management of the third stage. He wished to enter a plea for the second stage. The use of forceps at too early a stage caused small traumata which were dangerous. Damage could also be caused by allowing the second stage to last too long. In regard to Dr. Furber's statement about the bath, it was the custom in some hospitals to give the patient a hot shower bath with plenty of soap. In his opinion this was more suitable. In discussing the treatment, Dr. Donovan said that it was most important to exclude all intercurrent conditions, especially pyelitis. This was frequently the cause of a rise in temperature. It was a very simple matter to take a catheter specimen of urine and examine it for pus. When pyelitis and other conditions were excluded, he agreed with Dr. Morton that immediate action should be taken. In no other part of the body was dead matter, probably infected, allowed to remain undisturbed. There was no certain means of telling whether it was infected or not. The lochia in normal cases contained streptococci so frequently that investigation in this direction was useless. In blood culture a large field of possibilities was opened up and much good work could be done by pathologists. The patient should be anæsthetized and a thorough examination made. In the absence of peri- or para-metritis, two fingers should be put into the uterus and débris removed. If complete removal was impossible, the blunt curette could be used after localization of any masses by the finger. The uterus should then be douched and drainage and contraction assured by the insertion of gauze soaked in iodine. Posture of the patient would assist in drainage and the use of strychnine and ergot would aid contraction. When the condition was severe the use of saline solution *per rectum* would be found of great value. At the first sign of peritoneal involvement posterior colpotomy should be performed. This was a life-saving operation at this stage.

DR. NEVILLE DAVIS said that, as one of the post-graduate students, he wished to thank the several speakers for their remarks. Speaking as a general practitioner, he said that he regarded the case of every patient whom he attended in confinements, as potentially one of septicæmia. He always felt in regard to the application of forceps that, while it was wrong to interfere too early, much harm could be done by waiting too long, for bruising aided the supervention of septic material. It was his habit to examine the placenta thoroughly and if it was not complete, to insert his gloved hand into the uterus and make certain that it was empty. He saw his confinement patients daily and massaged the uterus himself. He made the nurse keep all the pads and he examined them himself. In the treatment of septicæmia in patients with a well contracted uterus, no good could be done with local treatment. More good would be done by administering plenty of fluid by mouth than by the rectum.

DR. H. A. RIDLER raised the question of *ante partum* sepsis. In patients who had a rise of temperature before delivery with no obvious cause, this might be the explanation in a few cases.

DR. A. MUSCIO referred to the necessity of treating such conditions as an eczema in the pre-natal clinic. He remembered a patient who had suffered from an extensive eczema of the vulva and vagina. She had had a retained placenta and had lost a lot of blood. He had cleaned the vulva as well as possible before removing the placenta, but the patient had developed septicæmia and died within a week. It was dangerous to use the term auto-infection. The germs certainly came from the patient, but it was the attendants who placed them where they ought not to be. When the prophylactic methods of cleaning the vulva, as practised at the Rotunda Hospital, were compared with the methods in use every day, it would be seen that the latter were far too perfunctory. The question of curettage or not was an old one. It had been debated twenty years before by Dr. Camac Wilkinson. Personally he liked to use the fingers, but the curette alone was not sufficient. The uterus was too big after confinement to clean out without using the fingers.

DR. JOHN FLYNN pleaded that the able and almost convincing work of Victor Bonney should be treated with more discrimination, more judicial calmness or at least with an attitude of suspended judgement till both sides of the question had been investigated. When the distinguished investigator had propounded his views at the Royal Society of Medicine, that the greater number of cases of puerperal sepsis were due to infection by organisms derived from the bowel, he had reported that he had encountered strong opposition. It was a curious coincidence that, though Fourneaux Jordon and Victor Bonney attacked this problem from different angles and their work, able and original in each case, had been separated by a decade of years, yet they had arrived practically at the same or similar conclusions, namely, that, while septic organisms might be conveyed from one patient to another, this was not the most common mode. At any rate, till further research

either confirmed or refuted these views, Victor Bonney's suggestion that the practical sterilization of the ano-perineal region might be effected by the use of a 1% solution of violet green, might be more widely adopted.

DR. CLYDE DAVIS thanked the President for an address which would be of much use to country members. He referred to Dr. D'Arcy's accouchement *par excellence* in well-equipped hospitals. The country practitioner looked forward to treating patients in nursing homes. Here, at any rate, some effort at sterilization was possible. Private houses were often the seat of so much dirt and filth that the relative infrequency of puerperal sepsis was really a matter for wonder. If the Government would establish maternity hospitals or maternity wards in connexion with existing hospitals, better results would be achieved. Dr. Davis referred to the fact that Gordon Ley, in the City of London Hospital, in most instances allowed patients with a temperature of 40° C. (104° F.) to carry on for a week without any interference with the uterus. The results obtained compared favourably with any he had observed elsewhere. Holland adopted the same line of treatment.

DR. KATE KNOWLES referred to a potential source of infection that often escaped notice. She stated that when a nurse put a patient on to a bed pan she separated the *labia minora* and washed from above downwards. In this way infected material could be introduced into the passages.

DR. T. FARRANRIDGE spoke of the treatment as carried out at the Royal Hospital for Women. In ordinary sapræmic types of infection conservative treatment was most successful. Patients with temperatures of 38.9° C. to 39.4° C. (102° F. to 103° F.) were not subjected to uterine examination. Postural treatment, such as lying on the face, combined with uterine tonics was often instrumental in the expulsion of lochia. Hot vaginal douches and massage were also useful in this regard. He had observed all forms of treatment in patients with the severe septicæmic type and had come to the conclusion that it was the patient who did the treatment. These patients were either going to get well or they were not.

DR. A. J. OPIE said that some effort ought to be made to eliminate the "Gamp." He referred to the fact that an old woman, seventy-two years of age, with a septic ulcer had recently undertaken to nurse a patient with a *placenta prævia*. He had written to the Board of Health, asking what could be done in the matter, but had received a reply that nothing could be done. A man who drove a motor-car or who piloted a small boat on the river, had to have a licence before he was allowed to do so, but any woman, whether qualified to do so or not, could take charge of the much more serious matter of child-birth. In regard to treatment, he was satisfied that the forceps were used far too frequently. This obtained more perhaps in the city than in the country. Nurses coming from the city had often remarked this fact. If so much was said about prophylaxis, why was it not practised?

DR. A. J. ASPINALL said that the Australian Trained Nurses' Association had tried for a very long time to have a Bill brought forward in regard to the registration of all nurses. A member of Parliament had assured him that Mr. Oakes had promised that a Bill of this nature would be introduced next session.

PROFESSOR A. E. MILLS said that three tutors in obstetrics had recently been appointed, one to each of the teaching hospitals. All students therefore would receive a better training than formerly. They would receive six weeks' instruction in obstetrics. Three weeks would be devoted to definite clinical instruction and three weeks would be spent in the management of lying-in patients at the hospital.

DR. T. W. LIPSCOMB, in reply to Dr. Aspinall, said that he and Dr. Barrington had gone to different ministers in previous years. They had received promises identical with that made to Dr. Aspinall, but nothing further had happened. A Bill dealing with the whole matter was on the stocks, but had not been launched, perhaps because such a measure did not bring in any votes. He thought that the discussion had been valuable. He had spoken from the standpoint of a general practitioner. For seventeen years he had been in general practice in an industrial centre in a place where where many "catch" midwifery cases had been encountered. The conditions here had differed materially from those encountered by men on hospital staffs. He had spoken of interference with the uterus at a much earlier stage than that seen by the members of staffs of obstetric hospitals.

In regard to the massage of the uterus recommended by Dr. Barrington he thought that this was possibly a great help. He was always rather afraid, however, of disturbing blood clot and possibly causing a pulmonary embolus. This was a possible danger and as such it outweighed the good that would result from massage. A clean uterus, as a rule, contracted well. Curetting should be done early, before it could be said that the uterus was big and boggy. It was necessary to be quite certain that it was clean and the finger could not be trusted for this. If he curetted the uterus, he did it under forty-eight hours from the onset of symptoms, when the temperatur rose to 37.8° C. (100° F.) or the pulse rate to one hundred or more and remained so for twenty-four hours. He agreed with Dr. D'Arcy in regard to washing out the uterus. He had lost his Boazmann's intra-uterine douche twelve years ago and was glad that he had done so. He agreed with Dr. Chenhall that no interference should be made with the leucocyte barrier. He had talked of interference before a leucocyte barrier was formed. It was not advisable to wait for the reports of swab testing or blood culture before curetting, for much valuable time would be lost by so doing. Dr. D'Arcy and Dr. Furber were keen on hospital treatment. He thought that the small nursing home had an important and valuable place in general medical practice. Nursing homes could be improved much by the men who sent patients to them. In regard to what Dr. Morton had said about the tying of the ovarian veins, this would have to be done only by an experienced operating surgeon. He had not meant to convey the term autogenous as explained by Dr. Worrall; the organisms came from the patient, but were introduced from the surface outside the passages. The risk of introduction by the attendant's hand was no small one. Dr. Worrall had spoken of differentiating between the sapræmic and the septicæmic patients. He (Dr. Worrall) spoke as a consultant and many of the patients seen by him were in a later stage of the disease than those referred to by himself. He had had no reason to regret the course of treatment that he had always adopted. He had not found that patients could retain more than three hundred cubic centimetres (half a pint) of fluid injected into the rectum when injected at regular intervals. He agreed with Dr. Gibson that the rise in pulse rate was the first sign of trouble, but thought that it was very important that the pulse should be taken by the doctor and not by the nurse.

DINNER TO THE FEDERAL COMMITTEE.

THE COUNCIL OF THE VICTORIAN BRANCH OF THE BRITISH MEDICAL ASSOCIATION entertained the members of the Federal Committee, the Directors of the Australasian Medical Publishing Company, Limited, and the Editor of THE MEDICAL JOURNAL OF AUSTRALIA at dinner at the Hotel Windsor, Melbourne, on February 8, 1923. DR. L. S. LATHAM, the President of the Victorian Branch, presided.

THE PRESIDENT entrusted the toast of the Federal Committee to DR. J. W. DUNBAR HOOPER, who spoke enthusiastically of the valuable work accomplished by the Committee. He referred to his recent visit to England and to the power of the Empire-wide organization to which they all belonged.

MR. G. A. SYME responded briefly and called upon DR. R. H. TODD, the Honorary Secretary of the Committee, to supplement his remarks.

DR. TODD entertained his audience with an interesting and instructive summary of the present position of the Branches of the Association in Australia and dealt in some detail with the subject of the incorporation of the Branches.

DR. S. STANLEY ARGYLE, Member of the Legislative Assembly of Victoria, was very happy in the manner in

which he proposed the toast of the Australasian Medical Publishing Company, Limited.

DR. W. H. CRAGO, the Chairman of Directors, in a short reply, referred to the fact that, as the Company was about to enlarge its activities very considerably, he felt it incumbent on him to relinquish his position. This was the last occasion on which he would appear as a Director and Member of the Company.

DR. W. N. ROBERTSON, C.B.E., spoke with sanguine expectation concerning the scheme for extension and referred to the valuable work done by Dr. Crago as Chairman of Directors since the inception of the Company.

THE EDITOR in his reply alluded to the courtesy, kindness and friendly help which Dr. Crago had extended to him during eight and a half years.

NOMINATIONS AND ELECTIONS.

THE undermentioned have been nominated for election as members of the New South Wales Branch of the British Medical Association:

HARWOOD, HORACE BARCLAY, M.B., Ch.M., 1922 (Univ. Sydney), Royal Prince Alfred Hospital, Camperdown.

FITZSIMMONS, JAMES PETER, M.B., Mast. Surg., 1921 (Univ. Sydney), Lyons Road, Drummoyne.

RYAN, WILLIAM EDMOND, M.B., Ch.M., 1921 (Univ. Sydney), 61, Ernest Street, Crow's Nest.

THE undermentioned have been nominated for election as members of the South Australian Branch of the British Medical Association:

ALTMANN, HERBERT FRANZ, M.B., B.S., 1922 (Univ. Adelaide), Adelaide.

FISHER, HARRY MEDCALF, M.B., B.S., 1922 (Univ. Adelaide), Adelaide.

JOSE, GILBERT EDGAR, M.B., B.S., 1922 (Univ. Adelaide), Adelaide.

MICHELL, CHARLES FRANCIS, M.B., B.S., 1921 (Univ. Adelaide), Adelaide.

SOLOMONS, BARNET, M.B., B.S., 1922 (Univ. Adelaide), Adelaide.

THE undermentioned has been elected as a member of the South Australian Branch of the British Medical Association:

ABBOTT, ARTHUR GORDON, M.B., B.S., 1922 (Univ. Melbourne), Kapunda.

THE undermentioned has been elected as a member of the Western Australian Branch of the British Medical Association:

CORBET, LANCELOT MILLER, M.B., B.S., 1922 (Univ. Melbourne), Perth Hospital.

Obituary.

WILHELM KONRAD RÖNTGEN.

IT has been announced that Professor Wilhelm Konrad Röntgen died last week. Information concerning the exact place and date of his death is wanting. Wilhelm Konrad Röntgen was born in Lennep, Rhenish Prussia, on March 27, 1845. He studied physics under Kundt at the University of Zurich and took his degree in the year 1869. He remained at Zurich as assistant to Kundt for a year and then went to work at the University of Würzburg, where he stayed for two years. In 1872 he entered the Strassburg University and in 1874 he secured the title of "Privatdozent," the first landmark in an academic career. For a short time he became Acting Professor of Mathematics and Physics at the Agricultural Academy at Hohenheim. Returning to Strassburg he was appointed "extraordinary" professor (titular professor) in 1874. In 1879 he was appointed to the chair of physics at the University of Giessen. Nine years later he was called to the University of Würzburg to occupy the analogous position. From an early stage he conducted much research work of considerable originality and importance. The majority of his studies were published in the *Annalen der Physik und Chemie*. Among his more important publications, the following may be mentioned: the exact determination of the conditions of the specific heat of air; the changes in the volume and in the refraction of di-electric bodies by electrolysis; the electro-dynamic action of di-electric bodies moving in homogeneous electric fields; the compressibility of fluids; the influence of pressure on certain physical qualities. Toward the end of 1895 Röntgen observed that when an electrical current was passed through a Crookes's tube, a strip of paper treated with barium-platino-cyanide held in proximity to the tube became fluorescent. This phenomenon was noted even if the Crookes's tube was covered with black paper. He recognized that the electric discharge in the Crookes's tube was producing an unknown form of rays which differed from cathodal rays. Later he found that the rays, to which he gave the name "X-rays," were able to pass through a variety of solid substances, such as wood and even thin sheets of metal. These rays had properties in common with light and ultra-violet rays of producing fluorescence and of inducing photo-chemical action. Soon after the discovery the question of the identity or otherwise of cathodal rays and X-rays occupied the attention of physicists in almost every country. Röntgen showed that X-rays were not deflected by the magnet, while cathodal rays were so deflected. The latter had very weak power of penetration of objects opaque to light rays, while X-rays possessed a high degree of penetrability. But he emphasized the fact that the two classes of rays were found side by side. Röntgen was able to demonstrate X-rays under favourable conditions from currents passing through Geissler tubes. In the next place, many attempts were made to determine the wave length of X-rays. At first it was suggested that the vibrations in ether were longitudinal, like sound waves in air, and not transverse, like light rays in ether. In order to determine this point, attempts were made to polarize X-rays. The early experiments of Röntgen were suggestive, but not definite. Similarly, the results of his observations on the wave lengths of X-rays were important only in clearing the way for more exact and more elaborate methods.

In 1896 he read a paper before a meeting of medical practitioners and physicists at Würzburg on his discovery. During the next year the interest of the medical profession was awakened in this great discovery. The first skiagrams were of hands and feet. The original apparatus was naturally crude and faulty. Röntgen soon recognized that it would be essential to collect the bundle of rays at one limited point in the tube, if sharp definition were to be obtained on the photographic plate. Within a couple of years he introduced the concave cathode and the small platinum target. Induction coils with or without the interposition of a Tesla transformer were employed and high tension alternating currents were used. In this way many excellent pictures were made before the end of the century. Röntgen himself was able to produce in a series of skiagrams the skeletal picture of the whole of the human body within two years of his first announcement. It is unnecessary at this time to discuss the work that has been carried out in the perfecting of this wonderful achievement in physics. It may be admitted that Wiedemann, before Röntgen, had recognized a peculiar form of rays emanating from electrical discharges *in vacuo*. It may also be admitted that Crookes, Hittorff and Geissler had produced vacuum tubes through which electrical current could be discharged and had shown that the physical phenomena in connexion with these electrical discharges were curious and needed further investigation and determination. Notwith-

standing these and other antecedent researches, it remained for Röntgen to discover that the rays emitted from a vacuum tube filled with electrical discharge differed from the known cathodal rays in their power of penetrating substances opaque to ordinary light rays. It was his achievement that led to the discovery of the photo-chemical action on sensitized plates. At first the medical profession looked to this discovery for a revelation in diagnosis. Soon it was shown that the rays were capable of exerting a damaging influence on cellular life. Even as early as 1897 experiments were undertaken to utilize this destructive power on bacteria. It was stated as a result of these early endeavours that X-rays were useless in attacking diphtheria bacilli, but that tubercle bacilli in guinea-pigs were killed by their means. To-day the therapeutic uses of Röntgen rays are very different from then. In a quarter of a century many earnest workers have sacrificed their lives to science by working incautiously with these wonderful but uncanny waves in ether. The apparatus has been changed beyond recognition and its every part has been perfected and altered. But we are still using X-rays discovered by Röntgen in 1895 for visualizing bones and organs within the living body and for treating pathological conditions. The understanding of the physics, of X, α, β and γ rays has been furthered by others, but it was Röntgen who rendered all this extraordinary advance in physics possible. It was a wonderful piece of scientific research and its author, who has died at the age of seventy-seven, was a wonderful man.

Books Received.

THE DIAGNOSIS AND TREATMENT OF HEART DISEASE, by E. M. Brockbank, M.D. (Vict.), F.R.C.P.; Fifth Edition; 1923. London: H. K. Lewis & Company, Limited; Crown 8vo., pp. xii. + 232, with 25 illustrations, including three plates. Price: 6s. 6d. net.

AN INTRODUCTION TO ELECTRO-THERAPY FOR THE USE OF STUDENTS, by Catharine Jameson; 1923. London: H. K. Lewis & Company, Limited; Crown 8vo., pp. xii. + 196, with 78 illustrations. Price: 6s. net.

ANÆSTHESIA IN CHILDREN, by C. Langton Hewer, M.B., B.S. (Lond.), M.R.C.S. (Eng.), L.R.C.P. (Lond.); 1923. London: H. K. Lewis & Company, Limited; Crown 8vo., pp. viii. + 111, with 31 illustrations. Price: 4s. 6d. net.

LATERAL CURVATURE OF THE SPINE AND ROUND SHOULDERS, by Robert W. Lovett, M.D., Sc.D., Boston; Fourth Edition, Revised; 1922. Philadelphia: P. Blakiston's Son & Company; Sydney: Angus & Robertson, Limited; Demy 8vo., pp. xi. + 217, with 172 figures in the text. Price: 12s. 6d. net.

MEDICAL DIAGNOSIS FOR THE STUDENT AND PRACTITIONER, by Charles Lyman Greene, M.D., St. Paul; Fifth Edition, Revised and Enlarged; 1923. Philadelphia: P. Blakiston's Son & Company; Sydney: Angus & Robertson, Limited; Royal 8vo., pp. xx. + 1,308 (and Symptom Index and General Index), with 14 coloured plates and 623 other illustrations. Price: 42s. net.

Medical Appointments.

THE nomination of the undermentioned to be the Medical Board of Western Australia has been gazetted: DR. H. T. KELSALL (B.M.A.), who is appointed President of the Board, DR. W. TEETHOWAN (B.M.A.), THE HONOURABLE DR. A. J. H. SAW, O.B.E. (B.M.A.), DR. T. L. ANDERSON, O.B.E. (B.M.A.), DR. R. C. MERRYWEATHER (B.M.A.), DR. R. C. E. ATKINSON (B.M.A.).

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DR. O. R. CORR (B.M.A.) has been appointed District Medical Officer and Public Vaccinator at Wyalcatchem, Western Australia.

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DR. E. F. FISHER (B.M.A.) has been gazetted as an Appointed Member of the Licensing Court for the Licensing District of Wyalong, New South Wales.

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DR. M. J. SYMONS (B.M.A.) has been appointed Honorary Consulting Ophthalmologist at the Adelaide Hospital, South Australia.

Medical Appointments: Important Notice.

MEDICAL practitioners are requested not to apply for any appointment referred to in the following table, without having first communicated with the Honorary Secretary of the Branch named in the first column, or with the Medical Secretary of the British Medical Association, 429, Strand, London, W.C.

BRANCH.	APPOINTMENTS.
NEW SOUTH WALES: Honorary Secretary, 30 - 34, Elizabeth Street, Sydney	Australian Natives' Association Ashfield and District Friendly Societies' Dispensary Balmain United Friendly Societies' Dispensary Friendly Society Lodges at Casino Leichhardt and Petersham Dispensary Manchester Unity Oddfellows' Medical Institute, Elizabeth Street, Sydney Marrickville United Friendly Societies' Dispensary North Sydney United Friendly Societies People's Prudential Benefit Society Phœnix Mutual Provident Society
VICTORIA: Honorary Secretary, Medical Society Hall, East Melbourne	All Institutes or Medical Dispensaries Australian Prudential Association Proprietary, Limited Manchester Unity Independent Order of Oddfellows Mutual National Provident Club National Provident Association
QUEENSLAND: Honorary Secretary, B.M.A. Building, Adelaide Street, Brisbane	Brisbane United Friendly Society Institute Stannary Hills Hospital
SOUTH AUSTRALIA: Honorary Secretary, 12, North Terrace, Adelaide	Contract Practice Appointments at Renmark Contract Practice Appointments in South Australia
WESTERN AUSTRALIA: Honorary Secretary, Saint George's Terrace, Perth	All Contract Practice Appointments in Western Australia
NEW ZEALAND (WELLINGTON DIVISION): Honorary Secretary, Wellington	Friendly Society Lodges, Wellington, New Zealand

Diary for the Month.

FEB. 20.—New South Wales Branch, B.M.A.: Executive and Finance Committee.
FEB. 22.—South Australian Branch, B.M.A.: Branch.
FEB. 22.—Brisbane Hospital for Sick Children: Clinical Meeting.
FEB. 23.—Queensland Branch, B.M.A.: Council.
FEB. 23.—Central Southern Medical Association, New South Wales: Goulburn (Annual).
FEB. 27.—New South Wales Branch, B.M.A.: Medical Politics Committee; Organization and Science Committee.
FEB. 28.—Victorian Branch, B.M.A.: Council.
MAR. 2.—Queensland Branch, B.M.A.: Branch.
MAR. 6.—New South Wales Branch, B.M.A.: Ethics Committee; Executive and Finance Committee.
MAR. 7.—Victorian Branch, B.M.A.: Branch.
MAR. 8.—New South Wales Branch, B.M.A.: Nomination of Candidates for Election of the Council.
MAR. 9.—Queensland Branch, B.M.A.: Council.
MAR. 9.—South Australian Branch, B.M.A.: Council.
MAR. 14.—Western Australian Branch, B.M.A.: Council.
MAR. 13.—New South Wales Branch, B.M.A.: Medical Politics Committee; Organization and Science Committee.

Editorial Notices.

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